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AN INDIVIDUAL CURVE OF LEARNING: A STUDY IN TYPEWRITING

BY BLANCHE M. TOWNE

Interest in the curve of learning seems to be increasing from year to year, with the result that an even larger number of studies are being published.

In 1897 Bryan and Harter (1) made their much-discussed studies on the 'Physiology and Psychology of the Telegraphic Language' and followed it two years later with their theory concerning the acquisition of a hierarchy of habits. In 1903 Swift (9) made an extensive study into the acquisition of a physical habit, a mental habit, and a complex of the two. These and other men working along similar lines found that the typical curve of learning was hyperbolic in form, increasing rapidly at first and then flattening out as practice was continued. In 1919 Chapman (7) published the results of an investigation of twenty students who were learning typewriting. He found that there was no curve to which individuals approximated, even though they reached the same degree of skill after the same number of practices; but that for many there was a rapid initial rise. It will be necessary to have the data from several hundred cases before we can really determine the characteristics of a learning curve. In order to secure from such data results, comparable in every detail, one must make the practice material, the practice conditions, etc., similar for all.

It was a desire to know into what sort of a curve my own learning process would resolve itself and how I should proceed that led me to keep the data from the first one hundred practices on a Corona typewriter, and later to continue the

practice for seventy-six days longer, making a second series of records. I had had no previous experience. Instructions were procured from a teacher in typewriting and the method outlined in 'Rational Typewriting' by Cutler and Sorelle was followed in detail. All but the last or twenty-fifth lesson, were completed in the first hundred days of practice. The lessons were on graded material. After six months and a half had elapsed, during which time I had used the typewriter but very little, I again began practicing and continued for seventy-six days. Seventy days were spent on the first hundred and eighteen pages of 'Essays in Exposition' by Kurts, Cory, Blanchard, and MacMinn, dealing with material of approximately the same difficulty. There were extracts from three essays by John Stewart Mill, three by John Henry Newman, and a part of one by Matthew Arnold. As these were manifestly more difficult than the material which I had ceased to use at the end of the first hundred days, for purposes of comparison, the last six days were spent on material of the same degree of difficulty as that of the hundredth lesson. Forty-five minutes, between seven and eight o'clock in the morning, were devoted to the work. The original plan was to make the practice days consecutive. This was made impossible by a four-day vacation, three long breaks occasioned by necessary absence from the city, and four Sundays, during the first hundred days of practice; and one break of two weeks and one of three during the last seventy-six days of practice. No attempt was made to work at maximum speed, but rather to proceed with the same amount of effort that would ordinarily be put forth upon any work. Introspections were written down almost daily. No collateral reading was done during the first practices, nor was any scoring done. Practically all the reading was done before the last series, and the results were tabulated from time to time.

The Curve

The correctness of the curve depends upon the uniformity of the practice periods, the time of day when they were taken, the amount of work done during the periods just preceding

them, etc. These were all rigidly watched and care was taken to avoid even slight variation.

The first four days were used in practicing the letters in the order in which they appear on the keyboard. The number of letters written was large and the errors few. This work served, therefore, merely to establish a 'set' for typewriting. The real work began on the fifth day, when words were first written. So, for the purpose of our curve, we shall consider the fifth day of our practice as the first day.

The curves described in most publications are based on the same materials, or materials of the same difficulty, written at regular intervals. Our use of material of increasing difficulty in the earlier series, makes it impossible to make any direct comparisons with such data. A study of it may, however, throw some light on our curve.

In the first curve of accomplishment we counted off five correct letters for each error. The space between the letters and the shift between the lines counted as one letter, while any letter, figure, or mark requiring the use of the shift-key received the value of two.

The curve has no characteristic shape. It rises slowly during the first forty-five days, seems to remain at a level for the next twelve days, and then rises rapidly again to the end of the first hundred days of practice, except during eleven days, when the curve seems to remain at the same level, a very high one. At the beginning of the second series of practices, the curve drops to a much lower level and maintains it for about fifty days. Then there is a decided drop for five days, followed by a marked rise when the level of the last plateau in the first series is reached. The last six days of practice reached a level not attained at any other time during the whole practice. The learning was not uniform but proceeded irregularly, attaining a high score only to fall back for a long or short period before attaining it or passing it again. Swift found that the period between high scores was short but later investigations seem to show that it may be long. Typewriting is such a complex process and affected by so many different varying conditions that it seems only correct

to expect this. Physical and mental condition, difficulty of material, use of new parts of the machine, speeding up beyond the average, etc., could scarcely interfere and aid in the same degree each succeeding time.

Explanation of the Curve of Learning or Accomplishment

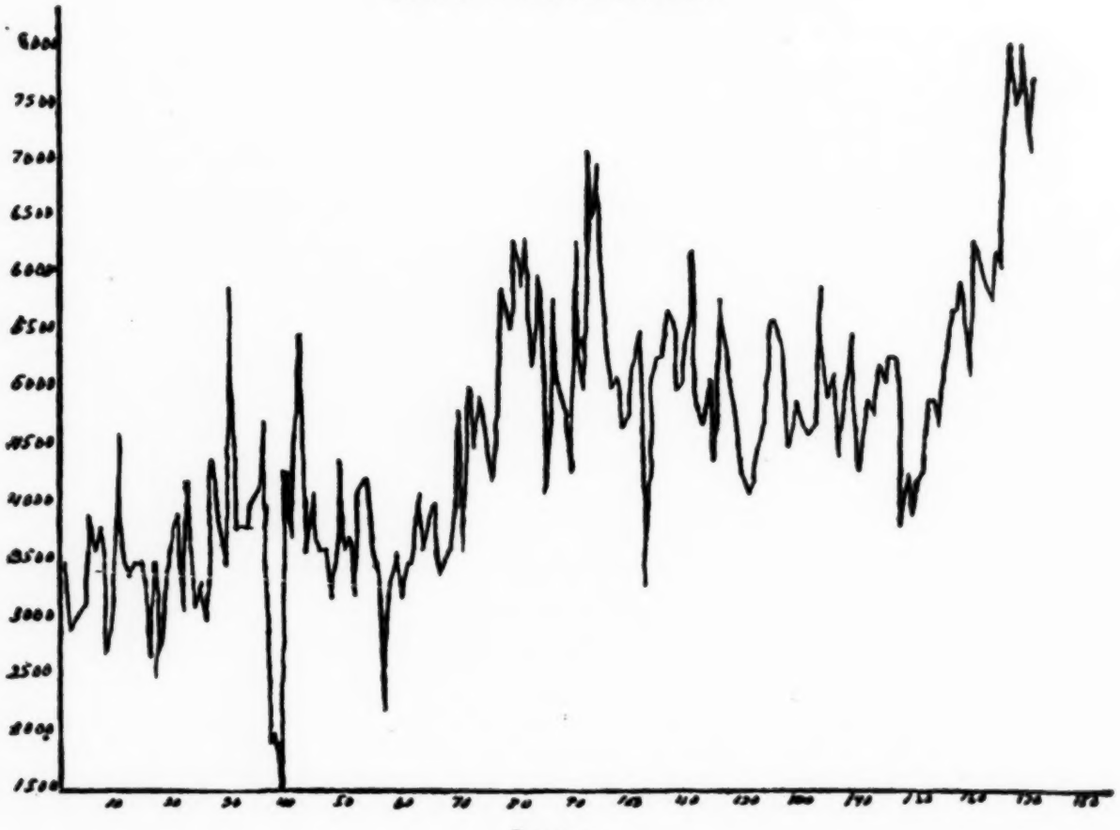
The rise on the fifth day was due to the repeated practice of a review exercise with very few errors. On the next day the drop was due to the new material's causing more errors, which occurred in groups and which left a very confused feeling. The sudden rise on the tenth day was due to the use of old material with few mistakes and to a feeling of exhilaration after having been detained from practice for a few days. A drop on the next day was caused by new material and the writing of many letters with many mistakes. The use of the shift key kept the curve down for the next few practices. Slow, painstaking, and careful work seems to account for the decided rise on the nineteenth, twentieth, and twenty-first days. During the next few practices I found myself confused whenever I tried to watch the copy. Up to that time I had been looking into space or closing my eyes instead of watching the writing. The movement of the paper and the little letters flying up and down annoyed me. Besides, I found myself stealing glances at the keyboard. This irritated me and I felt that I was making no gain in the work and that it had become a drudgery. The very low score made on the twenty-first day may be accounted for, in part at least, by the frequent use of the shift key for numerals. The next day the score was very little better, and the work was very exhausting. The same feeling continued during the nine days that I worked on this lesson. This probably came from the strain on the muscles. A feeling of calmness followed, and I worked slowly and carefully, reaching a higher score than previously except for the one attained on the thirtieth day. Then the beginning of a new lesson brought the score down again. I began to feel some pleasure in the work, and the weather became cooler. The more careful application which resulted

soon brought the number of errors down to one for the whole practice. However, the slow work reduced the number of letters and, in consequence, the curve dropped to a new low level. A new lesson requiring new parts of the machine kept it down for two more days. Up to this time there had been no plateaus at all. But when the curve rose again it remained at a level for about twelve days. This period exactly coincides with the time spent on lessons ten and eleven in the typewriting book. These contain the first whole paragraph and the first whole page to be written without repetition. The introspections reveal almost daily how great a difficulty this was. I was also emphasizing speed during this period. There is not one mention of pleasure and once a great dislike of the copy is expressed. This period of stress and strain left a feeling of discouragement, which persisted until the twenty-seventh lesson. Each day I had to drive myself to the practice. Even a perfect copy brought no desire to start another lesson. It was, however, just at this time that the score went up very rapidly. The second plateau, from the seventy-seventh day to the ninety-second, maintained a clear and distinct level, although a higher one than had hitherto been reached. Both decrease in the number of errors and increase in the number of letters written combine to increase this high score. The material, while in reality no easier, seemed so. The exercises were short and of more interesting content. The introspections contain many allusions to growing confidence and pleasurable feelings. For the first time I had actually become happy in the work. During the last few practices of the first series, the curve again started upward, accompanied by pleasurable feelings and self-assurance.

In the second series the drop, which we find is to be expected because of the much more difficult character of the material, both as to spelling, punctuation, and thought content, which, because of a habit of long standing, I could not divorce from the copying. The first fifty days were on the same level. I went to the task each day as one of my regular duties. I felt no false elation at the task nor any great depression that I was not gaining. It all seemed a part of the

day's work. Occasionally, I tried to spurt up but the gain would be of very little significance. Then I decided to put the emphasis on accuracy. The result of this was a decrease in the number of errors and also a decrease in the number of letters written, which, of course, caused a fall in the curve. Up to this time the score was being reckoned by dividing the number of letters written by the number of errors. As this seemed too great a price to pay for an error, it was decided to change the plan to the one which we have described above. Then I found my attitude changing from one of a certain watchful attitude toward errors to one which was a little more relaxed and which emphasized speed. This accounts for the following rise. Each day I felt a little freer than on the previous day, and, therefore, could speed up a little more. I, also, feel that this accounts for the higher level which was reached; a level, not very much higher than the preceding plateau but about proportionate to the relaxation from strain

CURVE OF ACCOMPLISHMENT



Letters

and control which was felt before. During the next six days of practice the material used was of the same difficulty as that used at the end of the first series. The result was what might have been expected. The curve rose a little higher than at any other time during the practice, and maintained that height during the period. This shows how little real gain was made during the previous seventy days of work, and indicates my level for the present, at least, in work of that difficulty. The level last reached on the difficult material is the one which shows the level of my capacity for such material.

Lower and Higher Order of Habits

At the outset the lower order habits dominated. Locating the desired key, placing the correct finger upon the key, and then striking it required individual attention. The letters 'a,' 's,' and 'l' soon needed no attention, while 'g,' 'b' and 'y' were still causing trouble on the sixtieth day of practice. 'X' was the one letter which had not become automatic on the last or one hundred seventy-sixth day of practice. 'As,' 'tion,' and 'es' were completely automatic before fully one half of the single letters had become so. By the end of the sixtieth day of practice, short, familiar words such as, 'and,' 'the,' and 'we' caused no trouble at all. The first group of small words were written automatically about the fortieth day. all lower order habits were steadily taking care of themselves in an ever increasing degree. Letters, syllables, words, and groups of words were dropping into the background. In the last few practices of the first series the thought of whole passages would be dominant and the lower order habits seemed not to interfere. These passages became longer during the second series, but strange combinations of letters in foreign or unfamiliar words brought the lower order of habits once more to the front.

Such observations as these completely disprove the theory that the lower order habits are completed before the higher orders are begun, or, indeed, that there is any regular order. Swift (9), Batson (5), and later investigators have reached the same conclusions.

Plateaus

Nature provides resting times in all her plans of life-processes. Therefore, it is not surprising that the learning process shows real resting places or plateaus. These may be interpreted as slumps in enthusiasm. Interest and attention wane at such times, energy seems entirely lacking, and the work is described as very tedious to the learner. A plateau often occurs after excessive effort or when the physical or mental condition is not good.

Bryan and Harter (2) thought that a plateau was a slowing up of the processes in order that certain lower order habits which were approaching their maximum development. could become sufficiently automatic to leave the attention free to attack higher orders of habits. The length of a plateau would thus measure the difficulty of making these lower order habits sufficiently automatic.

Book (3) thought that in learning typewriting there were two levels at which plateaus occurred. The first, so he believed, occurred when the speed was about one hundred ten strokes per minute and the second when it was one hundred forty-five strokes.

Chapman (7) found only a few short plateaus which he considered genuine. In the twenty cases that he studied, these plateaus did not occur at fixed places which were the same for each individual, but varied greatly. Neither Hill, Ryall, and Thorndike (8) nor Bradford (4) found any plateaus. They feel there is no need for these and that it is not necessary for habits to be developed and perfected in such a monotonous way, and that they do not 'suddenly shoot together into new combinations' and thus cause the curve to rise rapidly after a plateau. Peterson feels that the type of a curve drawn by some investigators does away with plateaus which would otherwise show up.

Swift (9) found that a small amount of effort to spurt up is helpful if it is not too severe. Overstrain exhausts the learner and hinders progress by bringing into consciousness those processes which help most when kept in the background.

The three plateaus in the curve which I have drawn from

my practices are each quite different in their characteristics. The first one is undoubtedly caused by the increased difficulty in the material. This did cause a big slump in enthusiasm. Interest and attention waned but the continued attempts to speed up do not indicate that energy was diminishing. It did not occur after excessive effort or when the physical or mental condition was not good. On the other hand the condition is reported as being better than it was at the beginning of the work. Therefore, this plateau could scarcely be called a true one in the sense that it is spoken of when it arises during work on material of the same difficulty.

The second is more nearly a true plateau than the first. The material was not markedly more difficult than that which had just preceded it, but came after what seemed to the learner a very strenuous period. Much effort had been used just to keep up the daily practices. The exercises themselves seemed long and tedious. At the beginning of the plateau the reaction to them became quite different. It may be likened to the pleasure of basking in the warm, spring sunshine after the winter's cold. Interest and attention were undoubtedly at a lower level during the eleven practices, for a high degree of energy and alertness is not compatible with the state of mind just described. Habits, too, were probably becoming automatized. In the practices that followed directly after the plateau they seem to have dropped away to such a degree that whole passages could be written with little or no attention directed to the actual running of the typewriter. Then, too, interrelations were fast becoming established. Near the end of the plateau the machine came to seem like a tool with but a single purpose instead of a composite with several ends to be attained before the written sheet could be produced. At no time during the period was I aware that the work was at a standstill. On the contrary, I thought that I was making more progress than at any time previous. Neither was I aware that my work was improving during the sudden rise which just preceded the plateau when it seemed that no progress was being made at all. Many investigators have noted that the judgment of the learner is

often very erratic. Contrary to the usual idea, the work at the time of the plateau was not tedious, but very enjoyable. But the variation in the material makes the comparison with other's results rather uncertain as to value. The level of my plateaus are not comparable with Book's findings because of the difference in the way the data was taken. They are more nearly comparable with those of Chapman who holds that individuals have their own individual levels for resting places. Possibly more effort could have been put forth during this second plateau so that the curve would have continued to rise had there been consciousness on the part of the learner that the work was at a standstill. Personally I find no regret that the first prolonged pleasure in the work was not interrupted. I believe that I was almost too self-satisfied to have been spurred on by any interest aroused by an outside force.

The third plateau occurred because I had reached the level of my ability, for work of the grade of difficulty used. Although the work of the last six days of practice on this material showed a somewhat higher level I feel certain that the whole plateau would have assumed this same level had the restraint been thrown off at the beginning of the plateau. Therefore, in the true sense of the word, this was no more a real plateau than was the first one.

Effect of Physical and Mental Condition upon the Curve of Accomplishment

Physical and mental condition apparently played a part in our results. Hot, sultry days seem to cause irritation, make the work unpleasant, and keep the curve down. A cool day usually brought the curve up and gave added enthusiasm for the work, all other things being equal. The physical exhaustion of the muscles kept the score down in several cases, especially using the shift-keys for the first time. The high points of the first sixty practices were accompanied by pleasurable feelings as also was the second plateau which maintained a much higher level than had been reached at any previous time. The pleasure which occurred at this time

when the learning was making no apparent progress may not seem so incompatible with the prevalent idea of displeasure during the plateau period when we remember that our plateau did attain such a high level.

Book's statement, that it is intense effort that educates, may be substantiated at least once during our practices. This was during the rise which just precedes the second plateau.

The practices of the second series were very little affected by physical or mental conditions, although the practices were continued during the same hot sultry months as were those of the first series. This was probably because so much of the work had become reduced to habit that other things had little or no effect upon the work. Intense effort did not raise the level of the third plateau, because, in a sense, the learning was complete. I had reached my level.

Periods of Absence

Hill, Ryall, and Thorndike found that the Sunday rest had no influence on the curve. A few learners gained and others lost by it.

The periods of absence or breaks in the regularity of practice occurred as follows:

After the 8th day of practice for 3 days.
After the 26th day of practice for 4 days.
After the 57th day of practice for 1 day.
After the 63d day of practice for 1 day.
After the 71st day of practice for 1 day.
After the 75th day of practice for 1 day.
After the 77th day of practice for 29 days.
After the 89th day of practice for 20 days.
After the 96th day of practice for 6 and ½ months.
After the 136th day of practice for 21 days.
After the 154th day of practice for 11 days.

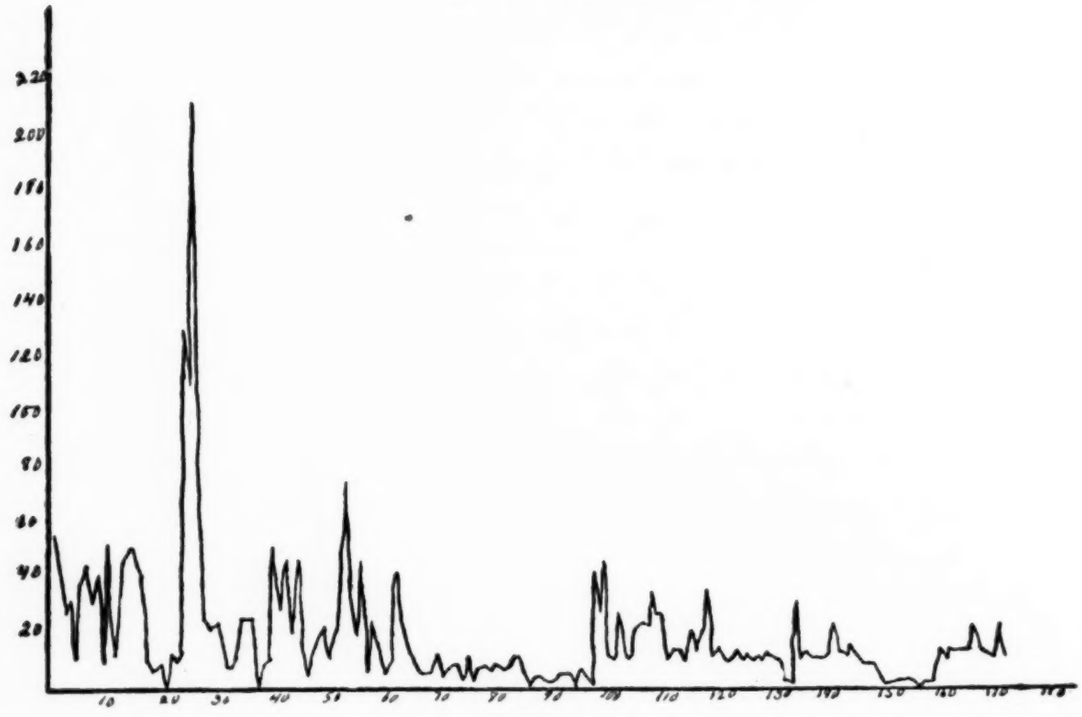
There was no perceptible loss or gain in the accomplishment even after long breaks of six and a half months, when the curve kept the level it assumed at the beginning of the series.

The Curve of Errors

The general shape of an error curve shows a rapid decrease at first. This becomes much more gradual as practice continues. It is not a smooth line, but is full of fluctuations.

Bergström found that there were fewer errors and a smaller number of keys hit, letters said, etc., when the rate was slower; but Thurston found no relation between accuracy and the rate of learning. Speeding up over the average is sure to result in a very large number of errors. One error is

Curve of Error



Errors

usually followed by several because the processes thus blocked result in a confusion. It, then, requires a little time to regain poise and composure. In his study on 'Psychomotor Mechanisms of Typewriting' Wells, also, found that there was a conscious or unconscious slowing up of all processes before errors.

My curve, based on material of increasing difficulty, shows only a very gradual decline toward a vanishing point, flattening more perceptibly after the sixtieth practice. The curve,

beginning with the second series, where the material was of the same difficulty, remained at the same level until emphasis was put upon accuracy, when it decreased toward a vanishing point. As soon as the emphasis was removed it again sought the level it had maintained through the practices of this series. I, therefore, seem to have a level for errors which is not the minimum possible, but which could be reduced at the cost of speed. The plateaus are practically coincident with the plateaus in the curve of accomplishment. One error was usually followed by several, especially during the first sixty practices, when most of the errors occurred. This was less true in the second series, as the knowledge which I had gained as to the psychical condition at this time usually helped me to stop and get possession of myself before continuing.

In general, the errors were caused by (1) new or unusual material, as groups of words, sentences, poetry, business contracts, etc.; (2) hard material which required the frequent use of shift-keys; (3) the use of new parts of the machine and (4) the speeding up above the general average number of letters per day for the lessons that had just preceded.

The per cent. of error for the first week was 11.69 and for the twentieth 1.35, a difference of ten. Thurston found that his group started with 58 per cent. of error and decreased to 36 per cent. at the end of twenty weeks. This is a difference of twenty-two. Here, as elsewhere, we are comparing two things which are not essentially alike.

Conclusions

1. Habits of all orders make progress simultaneously, some in a less and some in a greater degree.

2. A single, real plateau which was short and genuine occurred in the practice after the seventy-seventh day. The others were not real.

3. Intense effort, consciously or unconsciously applied, helps, all other things being equal.

4. The days of greatest accomplishment were generally accompanied by a pleasant feeling, except after a hard struggle against great odds, when the unpleasant feeling held over for a long time.

5. Breaks in the regularity of the practices seem to cause neither gain or loss in the net result.

6. The error curve decreased gradually for the first sixty practices and then proceeded more slowly toward the vanishing point.

7. There is a level in both the curve of accomplishment and the curve of error which is normal for me.

8. Errors were caused, (*a*) by new material, (*b*) by use of new parts of the machine, (*c*) by extensive use of the shift-keys, and (*d*) by speeding up over the general average.

9. Physical and mental conditions have an effect upon the results produced, the better the condition, the greater the accomplishment.

10. In graded material, work which is much harder than that just preceding, causes a slump in enthusiasm and a dislike for the subject in hand.

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EFFECTS OF LOSS OF SLEEP (II.)

BY EDWARD S. ROBINSON AND F. RICHARDSON-ROBINSON

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Introduction

The effects of loss of sleep may vary considerably from individual to individual. There is a suggestion of this fact in the results of our first experiment.[1] It was determined, therefore, to carry out this second experiment with a larger group of subjects in order to observe the general effect of loss of sleep upon a group and its more specific effects upon individual subjects.

Subjects and Method

The insomnia group consisted of 31 college students. Twenty-five of these, 3 women and 22 men, took all of the tests. The records of the other 6, being incomplete, were disregarded. This group of 25 will be referred to hereafter as the I., or Insomnia, group.

Our control group consisted of 39 college students, 2 women and 37 men. This group will be referred to hereafter as the S., or Sleep, group.

Both groups were given forms 5, 7, and 9 of the alpha intelligence examination in the order named, at the class hours, 10 A.M. or 11 A.M., on the mornings of three successive days. The alpha examination was employed because of its several nearly equal forms. The tests were scored according to the methods described by Yoakum and Yerkes.[2]

The members of the S., or control group lived *as usual* during the experiment. The members of the I. group abstained from sleep during the first day and night of the experiment, and until the completion of the second testing. No attempt was made to control their activities after that. Both groups were asked to state their usual hours for rising and retiring. These estimates afforded an average of 7.7 hours

[1] E. S. R. and Samuel O. Herrmann, Jour. of Exp. Psych., Vol. V, pp. 19–32.

[2] Army Mental Tests, 1920, pp. 53–79.

of sleep for the S. group and 7.4 hours of sleep for the I. group. For the night following the insomnia the S. group reported an average of 7.2 hours of sleep, and the I. group, 9.6 hours.

On the evening of the first day, upon which form 5 had been given to both groups, the members of the I. group gathered in the psychological laboratory. They had been asked to appear at the laboratory at eleven o'clock or soon thereafter. By midnight all the subjects were assembled. They had been given permission to bring work and play materials as they chose. They were also permitted the use of the various rooms, and they had soon chosen quarters for solitary or social pursuits. Sleep only was forbidden and doors were to be left unlocked for continuous inspections guarding against it. There was no evidence of attempts to sleep, and the restless ones who made continuous rounds during the night corroborated the reports that no one slept. At one o'clock a lunch was served. Work, conversation, and games filled the remainder of the night. At six-thirty in the morning, the students 'checked out.' They received instructions to take no sleep before the second testing. While their later reports showed that two students took 'cat-naps' of not to exceed a total of fifteen minutes each in their eight o'clock classes, the others had no sleep. At the class hour form 7 was given both groups. No instructions were given as to sleep during the following 24 hours, and on the third day both groups were given form 9. As we have stated, the I. group slept on the average 9.6 hours or 2.4 hours more than the S. group, between the testing on the second day and that on the third.

Test Records

Table I. gives the average examination scores of both groups for the three days of the experiment.

Table I

Averages (Test Scores)

Group	Day 1		Day 2		Day 3	
	Av.	P. E.$_{av.}$	Av.	P.E.$_{av.}$	Av.	P. E.$_{av.}$
Insomnia.....	160.20	±3.2	168.16	±3.0	168.48	±3.7
Sleep.....	161.00	±2.0	170.51	±1.7	171.54	±1.7

The scores of both groups increase from day to day, due, we may be sure, to the effects of practice in taking examinations of this kind.[1] Furthermore, there seems to be no significant difference in the amount of improvement shown by the two groups, even though one of them, the I. group, had no sleep between the testing on day 1, and that on day 2. What difference there is between the two groups favors the S. group, but it does not do so in any convincing fashion.

TABLE II

AVERAGES BY DAYS FOR BOTH GROUPS ON EACH TEST

Groups	Day 1	Day 2	Day 3
Test 1—Oral Directions			
Insomnia	10.36	11.28	11.36
Sleep	10.13	11.46	11.49
Test 2—Arithmetical Problems			
Insomnia	13.16	14.76	15.96
Sleep	12.79	14.62	15.67
Test 3—Practical Judgment			
Insomnia	12.36	13.24	13.72
Sleep	12.36	13.49	14.21
Test 4—Synonym-Antonym			
Insomnia	29.28	28.40	30.04
Sleep	30.41	29.74	30.79
Test 5—Disarranged Sentences			
Insomnia	17.44	20.44	19.04
Sleep	19.08	21.51	19.69
Test 6—Number Series Completion			
Insomnia	12.56	14.72	15.20
Sleep	12.79	13.85	15.36
Test 7—Analogies			
Insomnia	32.00	34.48	33.00
Sleep	29.82	34.95	32.90
Test 8—Information			
Insomnia	33.04	30.84	30.16
Sleep	33.69	30.92	31.44

[1] The amount and nature of these practice effects in the control group are discussed in this journal, Vol. IV, pp. 300–317.

A consideration of the results of the individual tests of the examination (Table II.) also inclines us to judge the differences between the two groups to be insignificant. On the whole, the records of the Sleep group show more improvement from day to day than those of the Insomnia group, but very slightly so. The Sleep group improved more or lost less than the Insomnia group in only 5 out of 8 tests between day 1 and day 2. Between day 2 and day 3, the Sleep group improved more or lost less in only 3 out of 8 tests.

Correlations

In order to ascertain whether the loss of sleep had affected the relative standings, or ranks, of the individuals of the I. group, the correlations were figured between the performances of different days. Table III. shows the values of

$$\rho\left(\rho = 1 - \frac{6\Sigma d^2}{n(n^2 - 1)}\right)$$

for both groups.

Table III

Coefficients of Correlations between Ranks on Different Days

	Insomnia		Sleep	
	ρ	P.E.	ρ	P.E.
Days 1 and 2.....	.86	.04	.75	.05
Days 1 and 3.....	.88	.03	.81	.04
Days 2 and 3.....	.91	.02	.84	.03

The coefficients are slightly higher for the I. group, indicating that shifts in ranking from day to day were less in the group subjected to an insomnia. In other words, there is no evidence that some of the individuals in the I. group were deleteriously and some advantageously affected by loss of sleep.

Feelings of Tiredness and of Effort

The students were asked before the examinations on each day to rate their feelings of freshness or tiredness, as follows: very fresh, fresh, as usual, tired, or very tired. Table IV. presents the results for these ratings.

TABLE IV

REPORTS OF FEELINGS OF TIREDNESS, GIVEN IN PERCENTAGES OF STUDENTS RATING EACH TIREDNESS ON EACH DAY

	Day 1		Day 2		Day 3	
	Insomnia	Sleep	Insomnia	Sleep	Insomnia	Sleep
Very tired.....	4	7.9	41.6	2.6	8	13.2
Tired.....	36	26.3	41.6	47.3	60	36.8
As usual.....	52	60.5	17.6	44.7	28	50.0
Fresh.....	8	5.3	0	5.2	4	0
Very fresh.....	0	0	0	0	0	0
Totals.....	100	100	100.8	99.8	100	100

It will be seen that on day 1, 40 per cent. of the I. group and roughly 34 per cent. of the S. group were more tired than usual, *i.e.*, tired or very tired. On day 2, 83 per cent. of the I. group and 50 per cent. of the S. group were more tired than usual, and on day 3, 68 per cent. of the I. group and 50 per cent. of the S. group were more tired than usual. In other words, the members of the I. group were but slightly more tired at the beginning of the experiment, felt much more tired after the insomnia on the second day, and on the third day were again near the stage of weariness represented by the normal group. It is apparent from the estimates of the S. group that the students' notions of what it is to feel *fresher* than usual were based upon a rather high standard of *freshness*.

Immediately after the examinations on each day, the subjects were asked to rate the amount of effort felt to have been put forth on the taking of the examination. The scale was as follows: best effort, good effort, ordinary effort, not much effort, and practically no effort. Table V. presents the results of these ratings.

On day 1, before the insomnia, 83 per cent. of the I. group and 90 per cent. of the S. group brought more than an ordinary amount of effort to bear upon the task, while a slightly greater percentage of the I. group than of the S. group put forth their best effort. Fewer of the S. group reported moderate effort, and none of either group on this day re-

ported less than moderate effort. On day 2, 75 per cent. of the I. group reported their best effort as over against 38 per cent. of the S. group. On day 3, approximately 50 per cent. of the I. group and 18 per cent. of the S. group were putting forth their best effort. There is some evidence that the S. group became bored as the experiment progressed. This seems to be indicated by the decreasing percentages giving 'best effort' and the appearance of positive values for 'not much effort.' This boredom was noticeable in the attitude of the class when the forms were produced on the third day.

TABLE V

REPORTS OF FEELINGS OF EFFORT, GIVEN IN PERCENTAGES OF STUDENTS RATING EACH DEGREE OF EFFORT ON EACH DAY

	Day 1		Day 2		Day 3	
	Insomnia	Sleep	Insomnia	Sleep	Insomnia	Sleep
Best effort.....	41.6	35.8	75.0	38.4	49.9	17.9
Good effort.....	41.6	53.8	20.8	46.1	29.1	58.9
Ordinary effort.....	17.6	10.2	4.2	12.3	20.8	17.9
Not much effort....	0	0	0	2.6	0	5.1
Practically none....	0	0	0	0	0	0
Total.....	100.8	99.8	100	99.4	99.8	99.8

The instructor asked for volunteers to take a fourth form of the examination on the fourth day, and the fact that twelve members of the I. group and two of the S. group appeared attested to the relative interest of the two groups in the experiment.

The ratings of effort, considered in conjunction with those of tiredness, suggest that loss of sleep causes a general lowering of feeling tone and that this effect is likely to be accompanied by an increase of effort put forth upon a test task. This increase of effort may so compensate for any ultimate loss in capacity that that loss may be relatively unobservable. Of course, it is possible that, although increasing tiredness and increasing effort appear, they are unrelated to the manner or result of test performance. We believe, however, that the theory that they compensate in

some degree for each other is more reasonable. Another closely related factor which may have minimized any actual loss in capacity suffered by the Insomnia group was the decreasingly interested condition of the S. group at the times of the second and third tests.

Effects of Loss of Sleep upon Individual Subjects

The subjects were requested to hand in statements of any specific effects of loss of sleep which they noticed. While there were marked individual differences, almost all reported some abnormalities. Nervousness, irritability, dullness, light-headedness, dizziness, and loss of motor control were prominent in the reports. Sleepiness was no more common than some of the other effects. Although unaware of the issues involved, a number of the subjects reported that their symptoms were in abeyance while they were taking the post-insomnia examinations. This, also, may be interpreted as indicative that the members of the I. group took unusual pains with the post-insomnia tests.

Conclusions

The qualitative results of this experiment, like those reported in our first paper, show that loss of sleep is marked by a variety of manifestations of disorganization which differ considerably from individual to individual, but which are prominent enough to be important. This finding accords with the results of previous investigators. Our attempts to measure the magnitude of the effects of loss of sleep for one or two nights by means of standard test procedures brought practically negative results. This does not mean, however, that the functions represented by the tests were unaffected, but merely that the effects were not too great to be compensated for by an extra amount of effort or interest, the existence of which our ratings of effort give good evidence. An interesting problem is furnished by the appearance of the extra effort in the Insomnia group. These subjects felt various symptoms of their condition as is shown by their

tiredness ratings and the more specific reports, and these may have stimulated that alteration in their method of work which we loosely describe as work with greater effort. There was every evidence, too, that the Insomnia group, working under unusual conditions, took interest in the experimental situation after the Sleep group, unaffected by loss of sleep, had become relatively bored. Perhaps a combination of these factors was operative.

We believe that our two experiments cast pronounced suspicion upon the procedure of placing too much emphasis in such studies as this upon test scores. Such data are far easier to handle than those obtained by less objective methods, but they are not therefore more significant.

PRACTICE EFFECTS IN INTELLIGENCE TESTS

BY EDWARD L. THORNDIKE,

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Yoakum and Yerkes ('20, p. 50) report an average gain of 8 points for a second over a first trial in Alpha. Dunlap and Snyder ('20, p. 395) report the gains of second over first, and third over second trial with Alpha (forms 6, 8 and 5 respectively), for 44 college students scoring 88 to 195 in the first trial. In a fourth trial (form 9) there was a large loss compared with trial 3, due, so they report, to decline of interest and effort. The median gain from trial 1 to trial 2 was about 15; that from trial 2 to trial 3 was about 12½. Allowing for the differences in difficulty of forms 6, 8 and 5 as per the data of Table 177, p. 660 of Psychological Examining in the United States Army, the median gain from trial 1 to trial 2 would be about 16 and that from trial 2 to trial 3 about 7.

Richardson and Robinson ('21, p. 302 and 305) find (using 39 individuals scoring 102 to 198 in the first trial) a gain of 9½ points for a second over a first trial of Alpha, and about 5½ points for a third over a second trial (1.03 as the actual gain in score plus 4.5 as the estimated greater difficulty of the form used in trial 3).

Combining the last two results we may expect a gain of about 12½ points for a second over a first Alpha, and of about 6 points for a third over a second, in the case of college students. As will be shown, we may expect that 10 minutes of fore-exercise could be used to reduce the former of these gains substantially.

The facts to be reported here are in general like the above. They differ in three respects. The number of cases studied is very large; the influence of inequality of difficulty of forms is usually eliminated by using two forms in changed order. Where allowances are made for differences of the forms in

difficulty, the allowances vary only from + 3 to − 3 and have probable errors of about 0.5. The test used was not the Alpha, but a test made up of harder tasks of the same sort plus five others, and given with all directions printed so that the person tested used as much or as little of the test time as he wished in mastering the directions. This test was prepared originally for use by the Examining Boards of the United States Air Service. It is printed in fifteen alternative forms. The scores of college entrants vary around 100 as a central tendency, ranging with few exceptions from 70 to 135.

Consider first the results when two forms are given in immediate succession after 10 minutes of fore-exercise spent in examining another form and doing the first two or three elements of each of the thirteen tests.

Forms *D* and *E* were given in the order *D E* to three groups of college entrants, numbering 312, 134, and 99 respectively. The median $E - D$ was $4\frac{1}{2}$, 4, and $3\frac{1}{2}$, respectively. The same forms were given in the order *E D* to 72 college entrants. The median $D - E$ was 12. The effect of the practice here was thus a median gain of 8.

Forms *I* and *M* were given in the order *I M* to 320 college entrants, 69 graduate students and 24 theological students. The median *M–I* was $3\frac{1}{2}$, − 1, and 2 respectively. As a weighted total result we have $M - I = 3-$. The same forms were given in the order *M I* to 419 college entrants and 409 normal-school students. The median *I–M* was $12\frac{1}{2}$ and $11\frac{1}{2}$ respectively. The effect of the practice was thus 7 +.

Forms *L* and *O* were given in the order *L O* to 273 college entrants. The median *O–L* was 8. When they were given in the order *O L* to 54 high-school seniors the median *L–O* was $8\frac{1}{2}$. The effect of the practice was thus 8 +.

Forms *N* and *F* were given in the order *F N* to 167 college entrants. The median *N–F* was $7\frac{1}{2}$; when the order was *N F* (70 entrants) the median *F–N* was 12. The gain from practice was thus 10−.

Forms *E* and *O* were given in the order *E O* to 33 first-year engineering students. The median *O–E* was 6. When given in the order *O E* to 20 students, the median *E–O* was 10. The gain from practice was thus 8.

A general weighted average of all these results gives 8 + with high reliability. Other scattered data confirm this estimate of 8 as the median gain from first trial to second trial after ten minutes of fore-exercise.

I have elsewhere shown that the practice effect from a first to a second trial when no fore-exercise is given is $12\frac{1}{4}$. The ten minutes of fore-exercise thus seems a useful proceeding. If, instead of it, a full regular trial of 30 minutes is given, the practice effect from a second to a third trial is $3\frac{1}{2}$.

I have no data for high-school graduates beyond a third trial, but by the kindness of Miss Henrietta Race, I have records of twenty gifted children about eleven years old, and nineteen children taken at random from the same school grade as the gifted children, who took fifteen forms in succession, one each school day. The average results, allowance being made for variations in the difficulty of the forms,[1] are as follows:

Trial	1	2	3	4	5	6	7	8
Gifted	87.5	108	109	109	106	113.5	109.5	110.5
Ordinary	51	76	67.5	79	82.5	80	78.5	80

Trial	9	10	11	12	13	14	15
Gifted	111.5	115	108	112	111	115	111
Ordinary	81	81.5	82	85.5	88	88	86.5

Combining the results for the two groups, and drawing a smooth curve to fit the observations (Fig. 1), it becomes obvious that the gain from practice falls off very rapidly and is, from about the fourth trial on, very small in comparison with differences between individuals. It does not, however, entirely vanish even in the fifteen trials. In so far as the results from so small groups may be trusted, it does almost vanish for the gifted group after about trial 6, as shown in Fig. 2. For them, indeed, all the practice after about the

[1] These allowances were computed from records of high-school graduates. They were for forms *A*, *B*, *C*, etc., in order, + 2, − 3, + 2, + 3, − 1, 0, − 1.5, + 1.5, + 2.5, + 1, 0, − 1.5, − 2, − 2, − 2.

second or third trial means only a very slight gain of about five points.

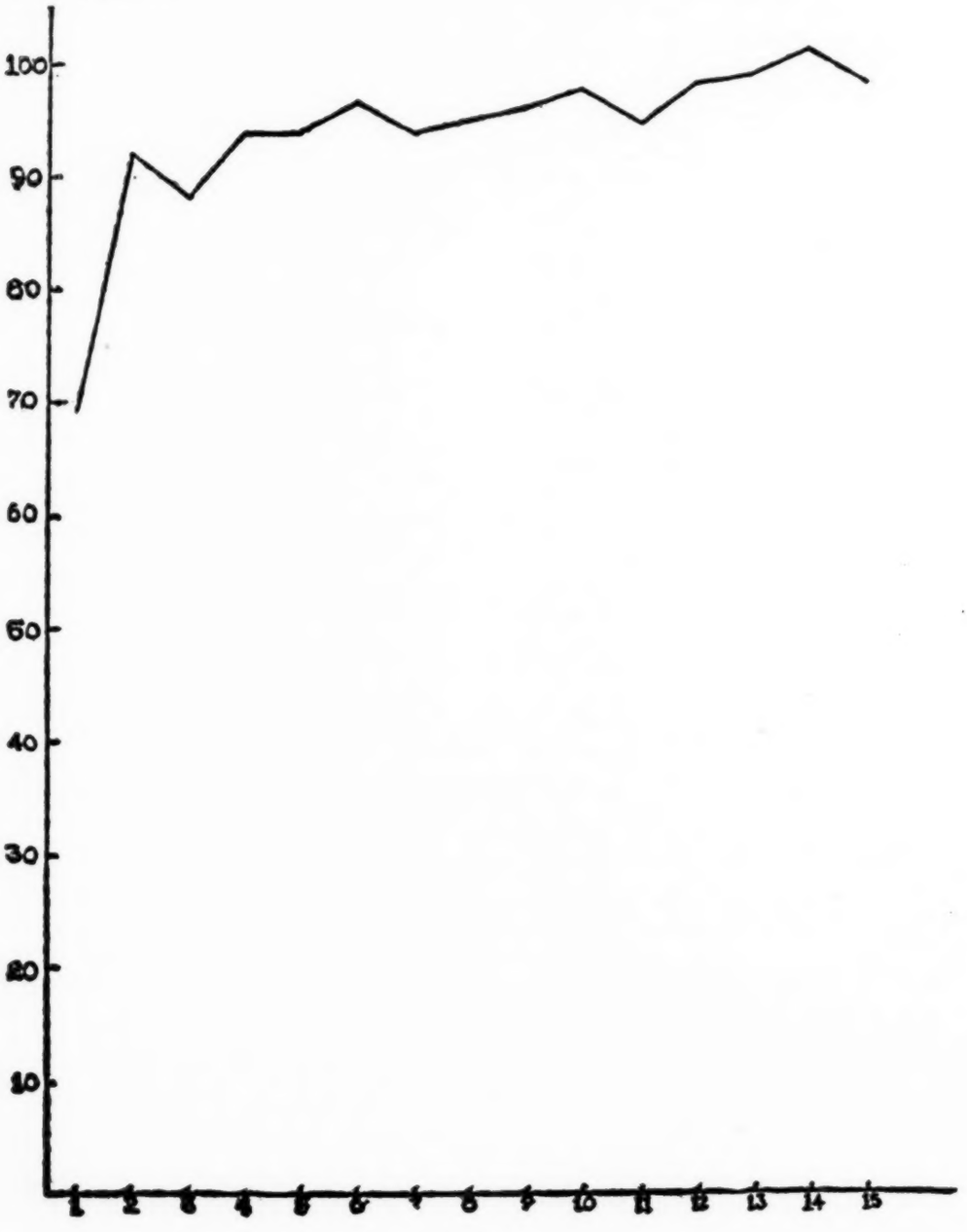

FIG. 1.

We may then hope that in experiments with individuals of the intelligence level of college students in respect to the influence of fatigue, drugs, distraction, and the like, the disturbing effect of practice after the third trial will be very, very slight. We may estimate the probabilities as follows (10 minutes fore-exercise being given):

	Per Cent.
Gain Trial 1 to Trial 2	8
Gain Trial 2 to Trial 3	3
Gain Trial 3 to Trial 4	1 to 1½
Gain Trial 4 to Trial 10 or 15	2

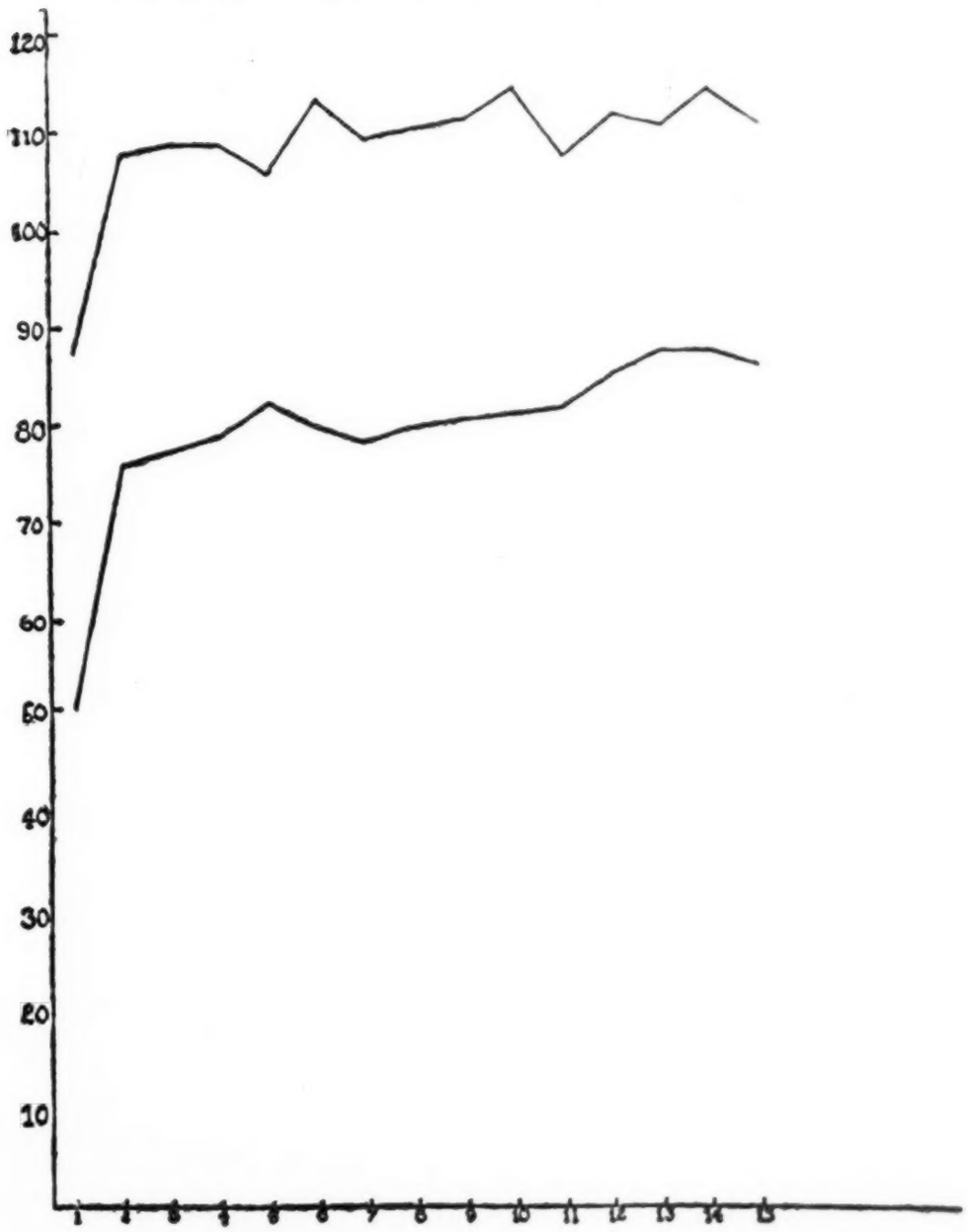

FIG. 2.

The practice effect is approximately the same regardless of the ability of the individual within the limits of 70 to 130. The correlations between intelligence score in the entire examination of 150 minutes and gain from first to second trial of Part I are:

467 high-school graduates, male..... −.11
254 high-school graduates, female..... −.15
222 normal-school students (mostly female)..... −.04

The correlations between intelligence score in Part I and gain from first to second trial of Part I are:

342 high-school graduates, female..... −.04
402 high-school graduates, female..... +.02

The more intelligent candidates thus made slightly less gross gain in the second trial over the first, probably because they grasp the nature of the tests better from the practice trial and first regular trial. In cases where we wish to infer what a second trial score would be from a first trial score, or to allow for practice in other ways, we may allow + 8 (or, a little more precisely, + 7, + 8, and + 9 respectively) to the brighter quarter, middle half, and duller quarter, of a group of the general level of high-school graduates.

In the preparation of future issues of the Intelligence Examination which is being used as one element in the record of candidates for entrance at Columbia University and elsewhere, we have studied tests of many kinds in respect to their freedom from influence by general practice in taking tests, special practice on the particular sort of test in question, and actual coaching. A report of this work will be made later, but it may be noted now that a test in reading difficult paragraphs and answering difficult questions about them, is certainly one of the very best tests in these respects. I find the practice effect very, very slight with university students, and Gates ('21, p. 381) has found the same with children in grades 4 to 8 of the elementary school. We are preparing a series of such tests to be of known, and approximately equal, difficulty suited to the intelligence level of college students, as the McCall-Thorndike series of ten forms is suited to the level of pupils in grades 3 to 10. These will be especially valuable for short-term experiments with fatigue, drugs and the like where it is not practicable to give an hour or so to fore-exercise. They have, however, two disadvantages in comparison with Army Alpha or the instrument discussed in this article. First, the test has to be rather long to secure

sufficient reliability (by sufficient variety in the topics of the paragraphs and otherwise). Second, the scoring requires judgment, and provision must be made to free the results from the personal equations of the scorers. We are now experimenting with a form where the individual *selects* the right answer instead of writing it, which will do away with this second disadvantage. It remains to be seen, however, whether this selective form will show the same high correlations with a criterion of intelligence that the customary form has shown.

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THE CONDITIONED PUPILLARY REACTION

BY HULSEY CASON

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SECTION I

Introduction

Much of the work which has been done on the conditioned 'reflex' is open to the criticism that the experiments involved 'voluntary factors' which complicated and in some instances vitiated the conclusions reached. We are not absolutely certain that all of the conditioned reactions which have been secured were 'reflexes' in a strict sense. The rôle which is played by 'conscious' factors, in the opinion of the present writer, looms up as a formidable factor. Much of the work of Pawlow and his associates, Watson's emotion experiment with the baby, and the latter's work on animals is strong argument in favor of the conditioned reflex being a real thing. The writer is of the opinion that the experiment now about to be described furnishes conclusive proof that a reaction may be conditioned without any direct aid from the 'voluntary factors.'

In beginning work on the conditioned reflex, it was our purpose to arrange an experiment which would eliminate these voluntary factors as far as possible. I was somewhat surprised to find that very few reflexes were suited to such an undertaking. Furthermore, it might be observed here that in almost any sort of conditioned reflex experiment, it is desirable (*a*) that the reflex used be one which can be called out repeatedly, (*b*) the reflex should be one which does not change markedly in appearance or fatigue easily with repetition, and (*c*) the reflex response should be adapted to objective registration. Just a little thought on the matter will, I think, convince the reader that there are few reflexes

which meet all of these requirements. The pupillary reflex, selected for this particular experiment, has the following very obvious advantages: (*a*) it can be readily evoked by a change in the intensity of light falling on the retina, (*b*) it does not fatigue markedly if proper precautions are taken, (*c*) changes in the size of the pupil can be easily observed with suitable apparatus, which I may say was available for the present experiment, (*d*) the fundamental stimulus (change in light intensity) calling out the pupillary reflex can be easily controlled, and (*e*), what is probably most important of all, this particular reflex cannot be controlled voluntarily in a strict sense. The fact that the pupillary reflex is not subject to voluntary control in a strict sense makes the conditioned pupillary reaction which was secured in this experiment both interesting and suggestive on theoretical grounds.

This experiment was carried out in the Columbia University laboratory, during the year 1921–1922. The writer wishes to make a very grateful acknowledgment to Professors R. S. Woodworth and A. T. Poffenberger, and to Mr. Arthur L. H. Ruben for many valuable suggestions and criticisms.

The 'fundamental' stimulus used was a change in the intensity of light falling on the retina. The 'conditioned' stimulus was generally the sound of an electric bell. The change in light intensity (followed of course by a change in the size of the pupil) and the bell stimulus were repeated together a large number of times. The purpose of the experiment was to see whether the change-in-size-of-the-pupil could not eventually be conditioned to the bell, that is to say, whether giving the bell stimulus alone could not eventually cause a pupillary reaction.

I was fortunate in having the use of an instrument which was constructed especially for measuring the size of the pupil and for controlling the light stimulus. This apparatus was devised by Karl Weiler in Munich, and is referred to as the 'Weiler apparatus.' I have included a brief description of its most essential features in section 3 of this report.

At the beginning of each experiment, I measured the natural effect of the bell stimulus on the size of the pupil.

The bell caused a slight dilatation in the case of every subject tested. Next followed a long period during which I would cause the pupil to make a marked change in size, by altering the light stimulus, and I would give the bell stimulus while this *change* was taking place. At the end of an experiment with a subject, I again measured the size of the pupil, with and without the bell, and found that the 'training' had left its mark. The change in light intensity, immediately followed by the bell stimulus, will hereafter be referred to as 'training stimuli.'

Two different procedures were used in giving these training stimuli. In one procedure, the light was turned *off*, and the bell stimulus was present during a *dilatation* of the pupil, as the training stimuli. It will be seen later that after this training a dilatation of the pupil was conditioned to the bell, *i.e.*, ringing the bell would now cause the pupil to dilate more than it had done at the beginning of the experiment. In the other procedure, the light was turned *on*, and the bell stimulus was present during a *contraction* of the pupil, as the training stimuli. After this training had been continued for some time a contraction of the pupil was definitely conditioned to the sound of the bell, *i.e.*, ringing the bell would now cause the pupil to actually contract. The same light which was used during the training period was always present when measurements of the size of the pupil were being made, both before and after training, and with and without the bell. The first procedure will be referred to as the 'training towards dilatation'; the second as the 'training towards contraction.'

The next section is a brief statement of certain general characteristics of the pupillary reflex which concern us particularly. The rest of the report is in the form of the usual laboratory experiment. Section 3 contains a short description principally of what the apparatus can do. Section 4 contains an outline of the procedure. In section 5 the results of all the experiments with the pupillary reflex are discussed somewhat in detail; and in section 6 the main conclusions which seem justified from the whole experiment are stated.

Section 2

The Pupillary Reflex

Of the two layers of unstriated muscle fibers of the iris, one layer is circularly arranged so that by its contraction it acts as a sphincter and causes the pupil to contract, the other layer is arranged radially so that by its contraction it causes the pupil to dilate. When light is thrown on the eye, there is an average latency in pupillary contraction of 0.2 sec. (19, 164). The pupil then makes a rather quick contraction, gradually slowing up until 0.87 sec. have passed. This is followed by some slight fluctuations; but 1.2 sec. after the original stimulus was received the pupil practically reaches a level (19, 168–169). These are only average figures, even normal people showing marked variations from them.

If a light of a certain intensity is falling on one eye, and a light of the same intensity is thrown on the other eye, both pupils will make a further contraction. Using very bright light, Weiler was able to produce a 'secondary reaction' of about 0.45 mm. (19, 167). Increasing the light for one eye caused an average contraction of 1.3 mm.; while increasing the light for both eyes caused an average contraction of 1.7 mm.

The pupil appears to make very fine reactions to a great many stimuli. The result in man is that the iris is probably always making fine, oscillatory movements (19, 111; 12, 185–248).

Some of the circumstances under which *contraction* of the pupil occurs are as follows:

1. Increase in the intensity of light. In man, the light falling on one eye causes simultaneous contraction of both pupils (19, 169; 11, 267). If a light which has caused a very large contraction of the pupil is continued, the pupil gradually relaxes with the adaptation of the retina to light.

2. Convergence of the visual axes (16, 73–100). A change in convergence, without a change in accommodation, is sufficient to provoke the pupillary reflex; the contraction being due solely to convergence, to the exclusion of the

modifications of accommodation (15; 12, 220–226). The movements of accommodation are simply associated with those of convergence (11, 80–81; 13, 22; 12, 225). These are the only causes of contraction of the pupil which concern us particularly, and it will be seen later that these two factors were controlled.

Some of the circumstances under which *dilatation* of the pupil occurs are the following:

1. Decrease in the intensity of light. Generally speaking, the size of the pupil is determined by the relationship between the adaptation of the eye and the illumination (13, 23). Almost complete adaptation for darkness occurs after 30 min., but if there is any light present at all the pupil contracts again as the eye becomes dark adapted.

2. Divergence of the visual axes.

3. Emotional states. According to Waller (17, 548), dilatation of the pupil may be provoked by painful impressions, suddenly aroused attention, and dyspnea. I conducted a short experiment to determine the effect of different mental states on the size of the pupil, designed to cover most of the conditions met with in this experiment, but I did not arrive at any very definite conclusions. Some careful research is needed to clear up this point.

4. Other sensory or sensorial stimuli (16, 101ff.; 12, 228–241). It seems that various stimuli, muscular, visceral, and sensorial, cause indirectly a dilatation of the pupil.

I shall not attempt to give a complete account of the nerve fibers which go to and from the iris, because this would take us somewhat afield, and is besides not necessary. The reader can find rather thorough descriptions in the works of Magitot (12), Weiler (19), Bach (1), Vidal (16), and Lutz (11). Bach goes more into detail than any of the others.

The size of the pupil, in normal people, is the result of a balance of forces maintained by the sympathetic nerves, which act as dilators of the pupils on the one hand, and the motor oculi which opposes them and causes contraction of the pupils on the other (6). If the sympathetic is stimulated, the pupils may dilate, because the balance is upset.

Conversely, irritation of the third nerve tends to contract the pupils. Thus the sympathetic is mydriatic in function, while the motor oculi is miotic. If the sympathetic fibers are stimulated, and at the same time the third nerve is inhibited, a considerable dilatation of the pupils may be expected. Since stimuli may affect the controlling nerves at various centers in their devious courses, it is to be expected that a change in the size of the pupil may be brought about by a variety of causes (6, 170). According to Magitot (12), two sympathetic centers, the one mesencephalic, the other bulbo-medullary, preside over the movements of the pupil, each through the medium of a peripheral ganglion: the constrictor center by the ganglion ciliare, the dilator centers by the superior cervical ganglion. It is thus a question of a centrifugal chain to two motor neurones.

In most of the experiments to be described, the 'conditioned' stimulus was a sound, generally of an electric bell. We are therefore concerned with the path of the cochlear nerve, which I shall not attempt to describe (9, 216).

Section 3

Apparatus

Through the kindness of Dr. George H. Kirby, Director of the Psychiatric Institute at Ward's Island, New York, I obtained the use of an instrument for measuring the size of the pupil and at the same time controlling the light stimulus. This instrument is known as the 'Weiler apparatus.' Weiler gave a very complete description of this apparatus in 1910 (19). In the same article may also be found an account of a systematic exploration of the pupil, for both normal and pathological people, together with a description of the accessory apparatus which he employed in photographing the movements of the iris and in recording the time of contraction and dilatation. I shall give only a brief description of the essential features of this apparatus (see Fig. 1).

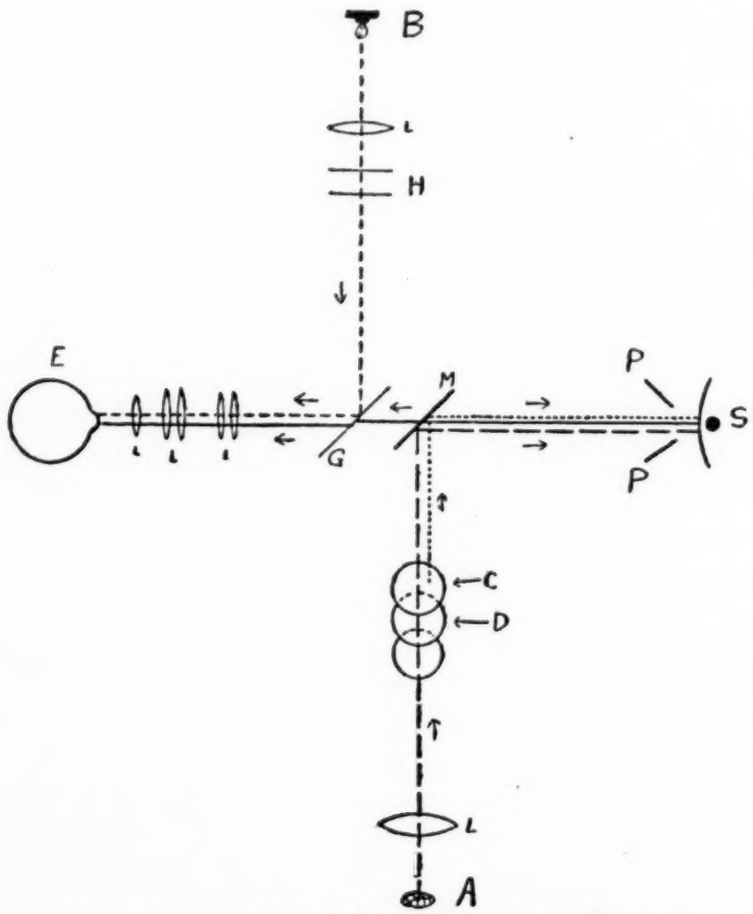

FIG. 1. Schematic Plan of the Weiler Apparatus. Not drawn to scale. *E*, experimenter; *S*, subject; *A*, main source of light; *H*, parallel hairs; *B*, light for parallel hairs; *M*, mirror; *G*, transparent glass; *D*, light filter; *C*, glazed paper which may be moved aside; *L*, lens. Light travels mostly in the direction of the arrows. The mirror (*M*) is to the side of the line of regard passing from *E* to *S*. The parallel hairs may be moved in and out by a thumb-screw. They seem to the experimenter to be superimposed on the subject's eye.

The experimenter is located on the left in the drawing (at *E*); the subject has a position on the right (*S*). Light from the two sources (*A* and *B*) is so controlled that it can travel only in the direction of the four broken lines of the figure. That coming from the bright source (*A*), which is enclosed in a box, is reflected by the mirror (at *M*) to the subject's eye. The luminosity of the light which reaches *S* can be regulated by light filters (at *D*). If we have a light of low intensity falling on *S*, a light of higher intensity may be readily secured by pushing in a spring button of the apparatus, which moves aside a piece of glazed paper (*C*),

placed horizontally, and at the same time makes an electrical connection which was generally used in this experiment to start the bell.

The mirror (*M*) can be revolved horizontally, directing the light from *A* through the subject's eyepiece (*PP*), and at the same time enabling the subject to see clearly the translucent paper (*C*), or the glass filter (*D*), as the case may be. The central portions of both *C* and *D* appear to the subject to be of uniform luminosity. The mirror (*M*) is just to the side of the line of regard from the experimenter (*E*) to the subject (*S*); this line passing through a piece of glass at *G*.

Light of low intensity (from *B*) comes down through a tube about 1.75 inches in diameter, passes 'by' the parallel hairs which appear to the experimenter to be placed verticalyl, and is reflected by the glass (*G*) to the observer (*E*), so that the hairs seem to the experimenter to be superimposed on the subject's eye. The intensity of *B*, and hence the luminosity of the outline of *H*, can be regulated by a rheostat, until the brightness of the outline of the vertical hairs is most favorable for seeing both the subject's eye and the hairs simultaneously.

The five lenses near *E* can be moved to the right and left (in the figure) so as to secure the most favorable focus on *H* and *S*. The small light at *B* can be moved up and down to assist the experimenter further in getting a focus on *H*. The vertical hairs (*H*) may be moved toward and away from each other by means of a large thumb-screw until they are each tangent to and on opposite sides of the subject's pupil. The diameter of the pupil is indicated automatically on a circular scale to the tenth of a millimeter. In the diagram, the lower right portion (*A–M–S*) of the whole apparatus is stationary. The left upper section (*E–G–B*) can be readily moved to the right and left, and up and down, until the vertical hairs are brought on the subject's eye. The subject keeps his head fairly steady by using the chin rest and at the same time resting the lower portion of the frontal bone against the projecting eyepiece.

From the above description, one might infer that the light stimulus can be regulated for only one of the subject's eyes at a time. The section *A–M–S*, however, is in duplicate,—one part for each of the subject's eyes. After measuring the size of one pupil, however, it is necessary for the experimenter to shift the part *E–G–B* to the right or left if the subject's other pupil is to be measured. The light stimuli for both eyes can be manipulated simultaneously; or the light for just one eye may be changed. In the case of every subject except *C* and *E* (first experiment only) I used only that part of the apparatus relating to the subject's left eye.

The 'make' and 'break' connections in the apparatus were generally used to start and stop the bell, at the same time regulating the light stimulus. Certain simple modifications in the electrical wiring were necessary, but these need not be described. The experiment was conducted in a dark room. This description is sufficient, I hope, to give an idea of the facilities I had at hand for measuring the size of the pupil.

Section 4

Procedure

The plan of procedure in this experiment required each subject to remain in a dark room continuously for a period of about 3 hours and 20 minutes. My obligations to those who were kind enough to serve as subjects are therefore very great. All of the subjects were graduate students in psychology except *Z*, who was a college freshman.

The results of 4 subjects, where the training was towards dilatation, are grouped together in the discussion of the results; and the procedure employed for these 4 subjects will now be described. This procedure was as follows:

1. At the 'ready' signal, the subject put himself in position and looked (with his left eye) at the light stimulus. The right eye was in almost total darkness.

2. After an interval of varying length, but always after the pupil had about reached a level of contraction, I turned

the light completely off, and gave the bell stimulus while the pupil was dilating. In this particular procedure, I had made the glazed paper (*C* in Fig. 1) opaque to light.

3. If the subject makes some particular movement each time the conditioning and fundamental stimuli are present, this movement may itself be 'conditioned' to something, and thus serve to complicate the results. Therefore, while the bell was ringing (and while the pupil was dilating), I had the subject remain in the same position and look in the same direction, without winking or otherwise moving the muscles of his face.

4. After a period of about 1 sec. the bell stopped ringing and the subject took a comfortable position to wait for the next 'ready' signal, which came 15 sec. later.

The results of 4 subjects, where the training was towards contraction, are likewise grouped together in the discussion of the results. The procedure used for these 4 subjects is as follows:

1. At the ready signal, the subject put himself in position to receive the light stimulus.

2. The light was turned on, and the bell stimulus was present while the pupil was contracting.

3. While the pupil was contracting the subject kept the same position and continued looking at the light.

4. When the pupil had stopped contracting (generally after about 1 sec.), I turned off the bell and the light together, and the subject rested until the next 'ready' signal was given.

For subjects *Y* and *Z* the light changes were made for both eyes instead of for only one. These two subjects, however, looked with the left eye at the luminous field on that side.

All measurements were taken with the same intensity of light, the same light which I turned on and off during the training series. That is to say, the change in light intensity was from a bright light to total darkness, or vice versa. This bright light is 'the light' which is so often referred to in this report, and it was constant throughout all experiments. It is to be particularly noted that this

same light was always *present* when measurements of the size of the pupil were being made. My purpose in having the change in light intensity from the bright light to total darkness, or vice versa, was to cause as large a *change* in the size of the pupil as possible each time the training stimuli were given.

Some preliminary experiments had indicated that to satisfactorily condition the pupillary reflex to the sound of a bell it would be necessary to have a rather long and continuous training series. For all subjects except *F* (in Table VII.) the schedule to which I rigidly adhered was as follows:

1. A period of 20 min. was allowed to elapse in the dark room, to take care of the adaptation factor (19, 154).

2. I took several measurements of the subject's pupil, with and without the bell, to determine the 'natural' effect of the bell on the size of the pupil. An individual measurement was executed about as follows: with the left hand, I moved the observer's telescope and the vertical hairs until the latter were 'over' the pupil. At the same time, with the right hand, I caused the two hairs to move in and out until they were tangent to and on opposite sides of the pupil. This operation generally consumed about 5 sec.[1]

The measurements of the size of the pupil, with the bell and without the bell, were mixed up in an irregular order and followed each other at intervals of about 20 sec. The reader may wonder why certain rests, etc., were of the stated length of time. Many of these were quite arbitrary on my part. My purpose was to cause as many changes in a sub-

[1] My personal error in measuring the size of a pupil was checked up by an objective method. There is no reason to believe that there was a constant error of any kind. Variable errors, however, in measuring the same pupil had a variability (S.D.) of 0.04 mm.; or, as the figures are used in the table, an S.D. of 0.4. This means that variabilities obtained throughout the experiment are given a little *higher* than they should be. If the obtained S.D., for example, of a distribution of measurements of the size of the pupil is 2.5, and if the variability (S.D.) of the measurements of the observer (personal variable error) amounts to 0.4, then the most probable S.D. of the distribution, if there had been no personal error in the observer (or, if compared with the 'true'), would be given by $\sqrt{(2.5)^2 - (0.4)^2}$, or 2.47. I do not make use of this correction in the computations to follow, although there is no reason why it should not be legitimately used.

ject's pupil as possible during a single experiment; but it is very probable that a better distribution of rest periods, etc., could be worked out.

3. A rest of 3 min.

4. The training series. This lasted 2½ hours, and consisted of 400 repetitions of the training stimuli. The schedule of rest periods, etc., which I followed here was as follows:

(*a*) 16 repetitions of the training stimuli, each repetition being followed by a rest of 15 sec.
(*b*) A rest of 1 min.
(*c*) The above, (*a*) and (*b*), repeated 5 times.
(*d*) A rest of 5 min.
(*e*) The whole group, *a*, *b*, *c*, and *d*, was itself repeated 5 times, making altogether a training period of 2½ hours.

During this training series, the bell stimulus was never present except when the pupil was changing markedly in the direction of the training. The reverse is also true: the pupil did not change markedly in the direction of the training except when the bell stimulus was present. That is to say, neither was present without the other. I did not take measurements of the pupil during the training series; the subject's eye being exposed to the bright light only long enough to secure the desired pupillary reaction.

For subjects *A*, *B*, *C*, *D*, *W*, *Y*, and *Z*, I used a small electric bell, driven by 4 dry-cell batteries, as the conditioning stimulus. This bell was screwed down to the table, and made a very aggressive sound. For subject *E* (in Tables III. and IV.), I used the same bell, but driven by one weak battery. For subjects *X* (in Table V.) and *B* (in Table VI.), I used the same bell again, but it was not fastened down to the table. For subject *F* (Table VII.), the conditioning stimulus was a weak but definite electric shock, applied to the right hand. For subject *X* (Table II.), I started off with a very loud electric gong, driven by 4 dry cells; but after 260 repetitions the clapper of this gong broke completely off, and could not be immediately replaced. I finished the experiment with the smaller bell, and the results were of the same general nature as those obtained from other subjects.

5. After the training series I again measured the size of the pupil, with and without the bell stimulus, to determine what the effect of the training had been. During these measurements, the bright light was always present. At this stage, the procedure was necessarily a little irregular. I generally took 2 or 3 measurements, immediately followed by several repetitions of the training stimuli. This had to be done because the conditioned reflex which I secured was rather fragile in constitution and soon passed out. In such a case as this, the total number of times the conditioned reaction is evoked is somewhat arbitrary. If you call it out only a few times the reliability of the result will probably not be very high; while if you call it out a large number of times the reaction disappears. So I tried not to be greedy in the matter, and took only a moderate number of measurements. These are shown in the various tables of results, with which we are next concerned.

Section 5

Results and Discussion

All of the 9 different subjects were graduate students in psychology except *Z*, who was a naïve subject. Six subjects: *B*, *C* (same as *W*), *E*, *F*, *Y*, and *Z* were men; and three subjects: *A*, *D*, and *X* were women. While there were 9 different subjects, there were 13 different experiments.

The results of the experiments with the first 8 subjects are shown in Tables I. and II. Table I. gives the results of 4 subjects, where the pupil was trained to dilate when the bell stimulus was given. Table II. gives the results of 4 subjects whose pupils were trained to contract when the bell stimulus was given. In every experiment, the bell caused a slight dilatation of the pupil at the beginning of the experiment. This dilatation varies with different individuals, and in the same individual at different times.

Let us consider subject *A*, in Table I., to illustrate the procedure. Before training, I measured this subject's pupil 7 times without the bell (distribution *a*), and 7 times with

the bell (distribution *b*). After training, I measured her pupil 11 times without the bell (*c*), and 11 times with the bell (*d*). I thus have four distributions. Consider now the two distributions before training, *a* and *b*. The natural effect of the bell is given by $b - a$, in this case + 1.14. This means that the bell increased the size of this subject's pupil 1.14 *tenths of a millimeter* at the beginning of the experiment. The P.E. of this Diff. is 0.56. I performed a similar operation on the two distributions after training. The bell now increased the size of the pupil 3.09, and the P.E. of this Diff. is 0.47. The effect of the training series on the size of the pupil is given by $(d - c) - (b - a)$, or + 1.95. While the reliability of the results in Table I. are fairly high, they are not conclusive unless they are considered in connection with Table II., where the pupils of 4 subjects were trained to actually contract when the bell stimulus was given.

In Table I. the conclusion seems established that a bell may be made to cause more dilatation after training (towards dilatation) than before. The conclusion that the bell will cause greater dilating effect on the pupil after training than before training is true for the 4 subjects, *A*, *B*, *C*, and *D* with reliabilities of 96 per cent., 86 per cent., 98 per cent., and 99 per cent., respectively. It is to be noted that the *same* positive conclusion was reached in the case of *each* of these 4 subjects with the reliabilities as just indicated. Now the reliability of the conclusion itself is obviously greater than any one of the the 4 individual reliabilities given above. In fact, the chances are 99.9997 out of 100 that the same conclusion itself is correct; and 81.55 out of 100 that the same conclusion would be reached again in *all* 4 cases.

Table II. gives the results of 4 subjects, *W*, *X*, *Y*, and Z, whose pupils were trained to contract. Let us consider subject *W* for illustration. Before training, the bell dilated the pupil 3.50. After training the bell *contracted* the pupil 1.73. The total drop is -5.23 ± 1.45; and the chances are 99 out of 100 that, if the experiment is repeated under the same conditions, the bell will have more effect in the

TABLE I

EFFECT OF TRAINING TOWARDS DILATATION

Subjects	*A*	*B*	*C*	*D*
Before Training:				
(*a*) Light:				
Meas.	7	9.	10	8
Av.	40.43 ±0.33	41.33 ±0.58	65.30 ±0.53	52.50 ±0.34
S.D.	1.29	258	2.49	1.41
(*b*) Light and bell:				
Meas.	7	9	10	8
Av.	41.57 ±0.45	43.67 ±0.30	69.40 ±0.32	56.25 ±0.55
S.D.	1.76	1.33	1.50	2.33
Bell, or $b - a$	+1.14 ±0.56	+2.34 ±0.71	+4.10 ±0.62	+3.75 ±0.65
Training: Bell with dilatation of the pupil				
After Training:				
(*c*) Light:				12
Meas.	11	12	9	54.25 ±0.46
Av.	38.82 ±0.39	41.67 ±0.33	52.00 ±0.76	2.35
S.D.	1.19	1.74	3.40	
(*d*) Light and bell:				
Meas.	11	12	9	12
Av.	41.91 ±0.27	45.33 ±0.24	59.67 ±0.48	63.08 ±0.38
S.D.	0.81	1.25	2.14	4.37
Bell, or $d - c$	+3.09 ±0.47	+3.66 ±0.41	+7.67 ±0.89	+883 ±0.85
Effect of training, or $(d - c) - (b - a)$	+1.95 ±0.73	+1.32 ±0.82	+3.57 ±1.08	+5.08 ±1.32
Chances out of 100 that $d - c$ will be equal to or greater than $b - a$	96.41	86.12	98.68	99.53

The numbers in the above table represent the horizontal diameter of the subject's pupil, to the tenth of a millimeter. The measurements of the pupil at the beginning of the experiment, with and without the bell, were mixed up together (distributions *a* and *b*). The measurements of distributions *c* and *d* were also mixed up with each other. For each distribution, the number of measures made, the average, the standard deviation, and the probable error of the average, are shown in the table. The effect of the bell on the size of the pupil is represented by $b - a$, before training; and by $d - c$, after training. The probable errors of these differences were also calculated. During the 'training series' the light was turned *off*, and the bell sounded while the pupil was dilating. This was repeated 400 times. Some measure of the *effect of this training* is obtained by subtracting the effect of the bell before training ($b - a$) *from* the effect of the bell after training ($d - c$). This gives a difference between two differences, or $(d - c) - (b - a)$. Finally, the reliability of this difference was obtained. For example, if $(d - c) - (b - a) = + 1.95 \pm 0.73$, the chances are 96 out of 100 that, if the experiment is repeated under the same conditions, $d - c$ will be equal to or *greater* than $b - a$. Or, stated in another way, the chances are 96 out of 100 that the bell will dilate the pupil *more* after training than before training, if the experiment is repeated under the same conditions.

TABLE II.

EFFECT OF TRAINING TOWARDS CONTRACTION

Subjects	*W*	*X*	*Y*	*Z*
Before Training:				
(*a*) Light:				
Meas.	4	4	4	5
Av.	57.50 ± 1.11	41.25 ± 0.69	41.00 ± 0.00	43.40 ± 0.79
S.D.	3.30	2.05	0.00	2.65
(*b*) Light and bell:				
Meas.	4	4	4	5
Av.	61.00 ± 0.63	43.00 ± 0.77	44.00 ± 0.41	47.40 ± 0.65
S.D.	1.87	2.12	1.22	2.15
Bell, or $b - a$	+3.50 ± 1.28	+1.75 ± 1.03	+3.00 ± 1.41	+4.00 ± 1.02
Training: Bell with contraction of the pupil				
After Training:				
(*c*) Light:				
Meas.	7	4	7	7
Av.	51.11 ± 0.48	42.25 ± 1.08	40.00 ± 0.33	38.85 ± 0.75
S.D.	1.88	3.21	1.31	2.95
(*d*) Light and bell:				
Meas.	7	4	7	7
Av.	49.28 ± 0.50	37.25 ± 0.51	39.90 ± 0.25	36.85 ± 0.87
S.D.	1.98	1.52	0.99	3.42
Bell, or $d - c$	−1.73 ± 0.69	−5.00 ± 1.19	−0.10 ± 0.41	−2.00 ± 1.15
Effect of training, or $(d - c) - (b - a)$	−5.23 ± 1.45	−6.75 ± 1.57	−3.10 ± 0.58	−6.00 ± 1.54
Chances out of 100 that $d - c$ will be equal to or less than $b - a$	99.25	99.81	99.98	99.57

In the above table, the statistical handling of the results was the same as in the case of Table I. The training, however, was in the other direction: towards contraction. For each of the 400 training stimuli, a light was turned *on* and a bell sounded while the pupil was contracting. $d - c$ here is negative, showing that after training the bell contracted the pupil in every case.

direction of contraction after training than before. After this training towards contraction, the effect of the bell was to contract the pupils of all 4 subjects. The conclusion is therefore justified that the training had *some* effect; and the individual reliability of this conclusion is over 99 per cent. in each one of the 4 cases. For all practical purposes, there are 100 chances out of 100 that the conclusion itself is correct; and 98 out of 100 that the same conclusion will be reached again in *all* 4 cases. The results of Table II. are fairly conclusive in themselves. However, it might be urged that negative auditory adaptation and fatigue could account for

some of the decrease in size of the pupil. No exact measure of the importance of these factors can be obtained. This makes it necessary to consider the results of Table II. in connection with those of Table I. In Table I. the bell caused the pupil to dilate *still more* after the training had been given, and the factors of fatigue and adaptation would not be sufficient to account for this effect. The fact that the pupil could be made to actually contract, as shown in Table II., is positive evidence that the emotional factor does *not* account for the results. Judging only from the results of Tables I. and II., there is a possibility that a *dilatation* of the pupil was not conditioned to the bell, since the emotional factor might account for the findings of Table I. This point is cleared up satisfactorily in later experiments, where we shall have more to say about this emotional factor. There is no doubt at all, however, that a *contraction* of the pupil was conditioned to the bell, because adaptation and fatigue cannot account for any contraction below zero when the bell stimulus is given.

The matters of negative auditory adaptation and fatigue had the *same* opportunity of operating when the training was towards dilatation (Table I.) as when the training was towards contraction (Table II.). After training, in the former case, the pupil dilated still more; after training, in the latter case, the pupil actually contracted. These two factors therefore cannot explain the effects produced.

One further possibility might be considered at this time. During the training towards contraction, the subject may have become accustomed to the often-repeated procedure: the bell ringing just after the light was turned *on*. Also, during the training towards dilatation, the subject may have become accustomed to the bell ringing just after the light was turned *off*. Near the end of either experiment, when the subject was being 'tested' for the conditioned reaction (*i.e.*, the bell stimulus present *without* the change of light), there is a vague possibility that there was an increase in emotional tone, due to this somewhat irregular procedure. I mention this possibility not because I think it is important, but simply

because someone might suggest it as an explanation for my results. According to the introspective reports of the subjects, taken at the end of the experiment in each case, they were not thinking about the matter to any extent. I am not willing to admit that this 'emotional factor' is sufficient to explain the results of the experiments where the training was towards dilatation, because the results were far too regular to be accounted for in this way. It is entirely out of the question to suppose that this factor could explain *any* of the results where the training was towards contraction, because in these cases (Table II.) the pupil actually contracted at the end of the experiment!

There is rather conclusive evidence from various sources that the size of the pupil cannot be changed voluntarily. I have tested this matter out in a short experiment, and obtained negative results. One occasionally sees a statement in the literature on the pupillary reflex that certain individuals can voluntarily change the size of the pupil. It is only claimed for very rare individuals, and even these claims are frequently discredited by writers and investigators of the subject. Since I succeeded in conditioning the pupillary reflex in every experiment which was tried, 13 in all, the chances are evidently exceedingly remote that *all* of these subjects were controlling their pupils voluntarily. Weiler, who has done perhaps the best work on the pupil with the serial photographic method, found that 'thinking' caused the pupil to dilate a little, whether the thinking be about dilatation or contraction. For further discussion of this subject, the reader is referred to the original article by Weiler (19, especially 181–182).

In a study which has been made of the connections between the cerebrum and the iris, by the electrical stimulation method, no evidence at all was found for cortical centers in the true sense of the term (3, 42). Magitot holds that there are no cortical centers of dilatation, but only non-specialized points of departure (12, 241).

Judging from the combined results of Tables I. and II., it is *certain* that the pupillary reflex was conditioned. This

is the most interesting consideration just now, on theoretical grounds. There is one further remote possibility, which I shall mention. It is possible that some of the subjects were occasionally 'thinking' about the changes which were taking place in their pupils, during the training series. For example, when the light was turned off, and the bell stimulus given during a dilatation of the pupil, subject *A* might have 'thought to herself' each time: "Now my pupil is dilating!" Later, the bell and the thought-of-dilatation might have become associated; so that eventually the bell was sufficient to cause a dilatation, via the subjective route. According to the introspective reports of the subjects, taken at the end of each experiment, they were thinking about the changes which were taking place in their pupils very little, either during the training series or at the end of the experiment. Perhaps the reader will agree that it is difficult to think about your own pupil changing at *any* time. Near the end of all the experiments, measurements of the pupil and the training stimuli were all mixed up together, and, according to the introspective reports of the subjects, they were thinking mainly about being in the right place at the right time. As further evidence against this supposition, it might be mentioned that subject Z in Table II. was a perfectly naïve subject. His pupil was definitely trained to *contract* when the bell stimulus was given. If this subjective factor was at all efficacious, which I do not grant, then the change in the size of the pupil was perchance not conditioned directly to the bell, but to a secondary reaction called out by the bell. If such were the case, the level of the conditioned reaction would be changed, but the fact of the conditioned reaction would not be affected.

In his presidential address before The American Psychological Association, in 1915, Watson made the following remarks about the conditioned reflex in general and about the pupillary reflex in particular: "I have dwelt at some length upon this subject for fear some might advance the view that the conditioned reflex is nothing more than the so-called 'voluntary reaction.' . . . The strongest argument

against such a point of view is the fact that it apparently can be set up on processes which are presided over by the autonomic system. To test this we have made a series of experiments having for their object the establishment of a pupillary reflex by the combined stimuli of a very strong light and a sound (bell). We found that the diameter of the pupil under constant illumination with fixation is very steady after the first five minutes; consequently it is possible to make measurements upon the pupil. To ordinary stimulations (sounds, contacts, etc.) there is a slight but not constant change in diameter (at times changes follow evidently upon intra-organic stimulation). But to such stimulation the pupil may respond either by dilation or constriction. In the short time which we had for training subjects we found two individuals in which, after fifteen to twenty minutes' training, the sound alone would produce a small constriction of the pupil in about seventy-five per cent. of the cases. In two subjects no such reflex could be built up in the time we had to devote to them." (18, 98–99). This quotation from Watson is given to show the importance which he placed in securing a conditioned pupillary reaction; and also to give a statement of the work which he did along the same line. His interest in this reflex was purely theoretical, and so also was mine.

The reader may have noticed that the statistical procedure used in Tables I. and II. is not absolutely rigid. For example, the training itself changed the absolute size of the pupil a little; so that it is not entirely correct to compare Diff. $d - c$ with Diff. $b - a$. As it is often stated, a Diff. of 80–70 is *less* than a Diff. of 40–30. At any rate, the chances are against 80–70 being *equal* to 40–30. These changes in level are comparatively small for all subjects except *C*, whose pupil was incidentally trained in both directions, 34 days apart. The 'level' of the pupil, before and after training, at the customary intensity of light but without the bell, is shown as follows:[1]

[1] A more rigorous mathematical procedure would be to express $b-a$ as a per cent. of $(a + b)/2$, and $d - c$ as a per cent. of $(c+d)/2$. If these differences were so

	Subject	Without the Bell	
		Before Training	After Training
Dilatation.....	*A*	40.43	38.82
	B	41.33	41.67
	C	65.30	52.00
	D	52.50	54.25
Contraction.....	*W*	57.50	51.11
	X	41.25	42.25
	Y	41.00	40.00
	Z	43.40	38.85

A comparison can be made between the 4 measures of $(d - c) - (b - a)$ in Table I., where the training was towards dilatation, and the 4 measures of the same in Table II., where the training was towards contraction. This will give the difference between the effect of training to contract and training to dilate. We have here two distributions of 4 measures each to deal with. The average is the best measure of the central tendency in these two distributions. For dilatation, the 4 measures of $(d - c) - (b - a)$ are $+ 1.95$, $+ 1.32$, $+ 3.57$, and $+ 5.08$. The average is $+ 2.98 \pm 0.49$. For contraction, the 4 measures are $- 5.23$, $- 6.75$, $- 3.10$, and $- 6.00$. This average is $- 5.27 \pm 0.46$. The difference between these two distributions is 8.25 ± 0.67; and the conclusion is certain that a difference in training will have some effect.

Perhaps enough has been said now to satisfy the reader as to the positiveness of these results. While the absolute

expressed, the reliabilities would be even *higher* than they are given in Tables I. and II. I hope the reader will not be disturbed at this statistical fallacy; because he would be even more disturbed over my descriptive language if I should use the more rigorous statistical procedure. The only real possibility of error which I see in the statistical handling of the results is the case where, in the 4 distributions, *a*, *b*, *c*, and *d*, of Tables I. or II., *a* is correlated with *c*, but not with *d*; or where *b* is correlated with *d*, but not with *c*. The formula for the probable error of a difference assumes that these correlations do not exist. If such correlations did exist, the probable errors of the differences, $b - a$ and $d - c$, are given too small, and the reliability would be less. But I have examined the original measurements very carefully, and find no reason at all for such a belief. The correlation has in fact the same chance of being positive as it has of being negative.

change in the size of the pupil, when evoked as a conditioned reaction, was relatively small, being on the average only about half a millimeter, the conditioned effect was very definite, and was secured in the case of every subject tested. While I do not think that a great deal of weight should be given to *introspections* in an experiment of this particular nature, there was one introspective report in particular which I think is at least worthy of mention. This observation was made by subject *B*, who is a good introspectionist. At the particular time in question, he had just finished the training series. His pupil had been trained to dilate when the bell stimulus was given. The procedure in his case had been to turn the light *off* and ring the bell while the pupil was dilating. The first time I tested for a conditioned reaction, *i.e.*, left the light *on* and rang the bell, he claimed that the intensity of the light increased suddenly the instant the bell began to ring. I observed at the same time that the pupil made a rather marked dilatation. The training stimuli were then repeated several times; and the next time I left the light on and rang the bell he made the same observation the second time, namely, that the light intensity seemed to suddenly increase the instant the bell began to ring. The explanation of this subjective effect is very simple. The bell dilated the pupil. This allowed more light to reach the retina, and caused the light to seem more intense. So it appears that the subject actually observed his own conditioned reaction this time. Although there is a possibility that some other subjective effect, rhythmical effect, or contrast effect may have entered into the situation, I am inclined to believe that the above explanation is the correct one. Subject *F* (Table VII.) also gave a similar introspective report.

My general plan of procedure in the above experiments was to have the nervous impulse from the bell stimulus arrive in the central nervous system at about the same time that the impulse to change the pupil was sent. I wished the auditory stimulus to be *present* when the nervous impulse was sent to the muscles of the iris. It appeared that this

would offer the best opportunity for the two impulses to become connected with each other. It would be very interesting to have some very definite information as to the best time for giving the conditioning stimulus, but this was not the main purpose of my experiment and I did not give much time to it. It would be interesting to know whether the bell would produce a greater conditioning effect if it was given a short time before the change in light, or if it was given during the change in size of the pupil; and also whether the bell would produce *any* conditioning effect if it was given after the fundamental stimulus. Although little has been said about this matter so far, data seem to be at hand on the basis of which a fairly definite conclusion may be reached. In the procedure of Table I., light caused a contraction of the pupil, and then the light was turned off and the bell stimulus was present while the pupil was dilating. In these experiments, dilatation became conditioned to the bell. Little if any connection was formed between the bell stimulus and the contraction of the pupil which immediately preceded it. At least it is certain that the dilatation which came *with* the bell had the 'right of way' over the contraction which came immediately *before* the bell, during the training series. For the 4 subjects of Table II., the light and bell were turned on at the same time, and the bell continued to ring for a little over a second while the light stimulus was contracting the pupil. After the pupil was contract*ed* the light and bell were both turned off together, and of course the pupil dilated. It is to be noted that in all 4 of these cases the contraction of the pupil, which came *with* the bell had the right of way over the dilatation which came immediately *after* the bell. From all of this it should be clear that the bell has a better opportunity of becoming conditioned to the pupillary reflex if it is repeated *with* it a large number of times, than if it is repeated *before* or *after* it.

Tables III. and IV. show the results of two experiments with the same subject, *E*, 2 months apart, which were de-

signed to test out this matter further. In Table III., the procedure was to ring the bell 0.5 sec. before and also during the contraction of the pupil. As soon as the pupil had contracted, both light and bell were turned off together, and the pupil of course dilated. A contraction of the pupil, which came 0.5 sec. before and also with the bell had the right of way over the dilatation which came immediately after the bell. I do not think that the very definite results in this experiment should be taken as evidence that the conditioning stimulus functions better when it comes before the fundamental stimulus; because exceptionally definite results were obtained in a later experiment on the conditioned eyelid reaction with this same subject. It appears that conditioned reactions can be established in this particular subject much more readily than in most people.

TABLE III.

SUBJECT *E*, JAN. 7, 8:55 A.M.—11:50 P.M.

Before Training	(*a*) Light	Meas.	20
		Av.	46.60 ±0.40
		S.D.	2.62
	(*b*) Light and bell	Meas.	20
		Av.	51.15 ±0.37
		S.D.	2.49
	Bell, or $b-a$		+4.55 ±0.54

Training: The bell rang for 0.5 sec. before the light was turned on, and continued to ring while the pupil was contracting. So the bell rang for 1.5 sec., and the pupil was contracting during the last second of this period.

After Training	(*c*) Light	Meas.	8
		Av.	46.38 ±0.60
		S.D.	2.50
	(*d*) Light and bell	Meas.	8
		Av.	44.00 ±0.62
		S.D.	2.60
	Bell, or $d-c$		−2.38 ±0.86
Effect of training, or $(d-c)-(b-a)$			−6.93 ±1.01
Reliability			99.99/100

In the above table, the training stimuli were repeated 400 times. This experiment is an effort to test the effect of having the 'conditioning' stimulus (the sound of the bell) come partly before the fundamental stimulus (light). Although the conditioning effect here is very marked, no general conclusion can be reached as to the best time for giving the conditioning stimulus on the basis of this experiment alone.

In Table IV. the procedure was as follows: (*a*) the light was turned on, (*b*) the light remained on until the pupil had contracted, (*c*) as soon as the pupil had contract*ed* the bell was rung for about 1 sec. while the pupil was in the contract*ed* state, (*d*) after this period the light and the bell were turned off simultaneously, and (*e*) the subject kept his same position for about a second while the pupil was dilating.

TABLE IV.

SUBJECT *E*, MCH. 6, 11:40 A.M.—2:50 P.M.

Before Training	(*a*) Light	Meas.	16	
		Av.	46.7	±0.70
		S.D.	4.15	
	(*b*) Light and bell	Meas.	16	
		Av.	47.9	±0.50
		S.D.	2.96	
	Bell, or $b - a$		+1.2	±0.86

Training: The light was turned on, causing the pupil to contract. As soon as the pupil had contracted, the bell sounded for 1 sec.; after which the bell and the light were both turned off. Thus at every repetition the bell came *between* the contraction and the dilatation; immediately *after* the contraction, and immediately *before* the dilatation.

After Training	(*c*) Light	Meas.	16	
		Av.	41.7	±0.51
		S.D.	2.05	
	(*d*) Light and bell	Meas.	16	
		Av.	44.6	±0.47
		S.D.	2.81	
	Bell, or $d - c$		+2.9	±0.69
Effect of training, or $(d - c) - (b - a)$			+1.7	±1.10
Reliability			85.14/100	

In the above table, the training stimuli were repeated 400 times, as in all former cases. This experiment is an effort to test the effect of having the 'conditioning' stimulus (the sound of the bell) come before or after the fundamental stimulus (light). Apparently a dilatation of the pupil, which in the training came *after* the conditioning stimulus, became conditioned to the bell. No general conclusion, however, can be reached from this experiment alone.

This whole operation completed one trial. So the bell stimulus was present for about 1 sec. *between* a contraction and a dilatation of the pupil. It was present immediately *after* a contraction, and immediately *before* a dilatation. There seemed to be a tendency here for the dilatation, which came

after the bell, to have the right of way over the contraction which came *before* the bell. I also do not consider that this experiment gives any very reliable evidence in favor of having the conditioning stimulus come before the fundamental stimulus. From the result of Table IV., it is suggested that the bell is not connected with a *state* of the pupil, but rather with a *change* in the size of the pupil. The bell stimulus was present while the pupil was *contracted*, and not while it was contracting or dilating. The bell was not connected with a contraction of the pupil, even though it was present each time the pupil was in a *contracted state*.

To sum up these various results on the question of the most advantageous time to give the conditioning stimulus, it might be stated (*a*) that *every* evidence is found for the forming of a connection when the bell stimulus is present *during* a change in size of the pupil; (*b*) no conclusive evidence is found for an increased effect when the bell comes *before* the change in the size of the pupil; and (*c*) positive evidence is found against a connection being formed when the bell stimulus is given *after* the change in size of the pupil, or, stated another way, against the forming of associations in the backward direction. The results shown in Tables V., VI., and VII. give further evidence in favor of these same conclusions.

Table V. shows the results of an experiment with subject *X*. The light was turned on and the bell stimulus was present while the pupil was contracting. As soon as the pupil had contracted, the light and bell were turned off together, and a telephone receiver sounded while the pupil was dilating. These training stimuli were repeated 400 times. The bell was the same as in former experiments. Current from the secondary coil of an induction apparatus was used to operate the telephone receiver. The receiver was fastened to the table, and made a harsh noise. The bell was not fastened down, and made a 'clear' sound. The 'pitch' of the receiver was a little higher than that of the

TABLE V.

SUBJECT *X*, MCH. 3, 7:30 P.M.–11:00 P.M.

Before Training	(*a*) Light	Meas.	12	
		Av.	48.9	±0.61
		S.D.	3.15	
	(*b*) Light and bell	Meas.	12	
		Av.	49.4	±0.70
		S.D.	3.61	
	(*c*) Light and receiver	Meas.	12	
		Av.	50.7	±0.72
		S.D.	3.7	
	Bell, or $b - a$		+0.5	±0.93
	Receiver, or $c - a$		+1.8	±0.94
	Receiver—bell, or $c - b$		+1.3	±1.00

Training: Towards contraction (bell), and towards dilatation (receiver).

After Training	(*d*) Light	Meas.	11	
		Av.	44.2	±0.31
		S.D.	1.53	
	(*e*) Light and bell	Meas.	16	
		Av.	42.2	±0.57
		S.D.	3.40	
	(*f*) Light and receiver	Meas.	15	
		Av.	47.1	±0.42
		S.D.	2.36	
	Bell, or $e - d$		−2.0	±0.65
	Receiver, or $f - d$		+2.9	±0.52
	Receiver—bell, or $f - e$		+4.9	±0.71

Effect of training (bell), or $(e - d) - (b - a)$	−2.5	±1.13
Reliability	93.21/100	
Effect of training (receiver), or $(f - d) - (c - a)$	+1.1	±1.07
Reliability	75.65/100	
Effect of difference in training, or $(f - e) - (c - b)$	+3.6	±1.22
Reliability	97.70/100	

For each of the 400 training stimuli of the above table, the bell stimulus was present while the pupil was contracting, and immediately following this, a noise was made in a telephone receiver while the pupil was dilating. Numbers in the table represent the horizontal diameter of the subject's pupil in tenths of a millimeter. The measurements of distributions *a*, *b*, and *c*, and likewise those of distributions *d*, *e*, and *f*, were mixed up together. The effect of training towards contraction, with the bell, and towards dilatation, with the receiver, may here be compared in the same subject.

TABLE VI.

SUBJECT *B*, MCH. 4, 7:30 P.M.–11:20 P.M.

Before Training	(*a*) Light	Meas.	9	
		Av.	40.4	±0.30
		S.D.	1.34	
	(*b*) Light and receiver	Meas.	11	
		Av.	42.0	±0.17
		S.D.	0.85	
	(*c*) Light and bell	Meas.	12	
		Av.	41.4	±0.27
		S.D.	1.38	
	Receiver, or $b - a$		+1.6	±0.34
	Bell, or $c - a$		+1.0	±0.40
	Bell—receiver, or $c - b$		−0.6	±0.32

Training: Towards contraction (receiver), and towards dilatation (bell).

After Training	(*d*) Light	Meas.	10	
		Av.	43.4	±0.31
		S.D.	1.43	
	(*e*) Light and receiver	Meas.	12	
		Av.	40.1	±0.34
		S.D.	1.75	
	(*f*) Light and bell	Meas.	10	
		Av.	48.2	±0.60
		S.D.	2.82	
	Receiver, or $e - d$		−3.3	±0.46
	Bell, or $f - d$		+4.8	±0.68
	Bell—receiver, or $f - e$		+8.1	±0.69
Effect of training (receiver), or $(e - d) - (b - a)$			−4.9	±0.57
Reliability			100/100	
Effect of training (bell), or $(f - d) - (c - a)$			+3.8	±0.79
Reliability			99.94/100	
Effect of difference in training, or $(f - e) - (c - b)$			+8.7	±0.76
Reliability			100/100	

For each of the 400 training stimuli of the above table, a sound was made in a telephone receiver while the pupil was contracting, and, immediately following, a bell stimulus was present while the pupil was dilating. The general plan of procedure is the same as in Table III., except that contraction is conditioned to the receiver, instead of to the bell, and dilatation is conditioned to the bell, instead of to the receiver. The measurements comprising distributions *a*, *b*, and *c*, and likewise those of distributions *d*, *e*, and *f*, were mixed up together. The effect of training towards contraction, with the receiver, and towards dilatation, with the bell, may here be compared in the same subject.

bell. The results show that the bell actually contracted the pupil after training. The receiver dilated the pupil after training even more than before. An interesting result of this experiment is that the pupil 'distinguished' between the bell and the receiver. It seems probable that 'just *any* stimulus' would not have produced this effect; because contraction was conditioned to the *bell*, and dilatation to the *receiver*. A comparison may be made between the effect of training with the bell and the effect of training with the receiver. This difference is represented by $+ 3.6 \pm 1.22$, and the chances are 97 out of 100 that some difference would be obtained again.

Table VI. shows the results of an experiment with subject *B*, in which the procedure was the same as that used in Table V., except that the stimuli were 'reversed.' These two experiments should be considered together. In Table VI., the light was turned on, and the receiver sounded while the pupil was contracting. When the pupil had contracted, the light was turned off and the bell stimulus was present while the pupil was dilating. The same bell and the same receiver were used here as in Table V. In Table VI., contraction was definitely conditioned to the receiver; and dilatation definitely conditioned to the bell. The 'neural mechanism' again distinguished between these two sounds. The results do not indicate whether it was a difference in pitch or quality or intensity of the sounds which was distinguished. It is probable that all of these elements played a part. The contracting effect showed up more markedly than the dilating effect in both cases. This same tendency, for contraction to show up more markedly than dilatation under the conditions of this experiment, had also been observed in almost all of the other experiments. Apparently the constrictor system is more readily innervated, and the dilator system more readily inhibited. The combined results of Tables V. and VI. remove all possibility of adaptation, fatigue, or the emotional factor explaining the effects produced. The results with subject *B* were more positive than those with subject *X*. This may or may not be due to the difference in procedure.

It is in all probability simply due to an individual difference in the subjects.

The combined results of Tables V. and VI. apparently furnish rather conclusive evidence that a connection was also formed between a *dilatation* of the pupil and the conditioning stimulus. The emotional factor operated just the same when the conditioning stimulus caused a contraction, as when it caused a dilatation of the pupil. Likewise, the 'emotional factor' functioned the same way in either case. A contraction of the pupil was secured *in spite of* the emotional factor; and a dilatation of the pupil was secured *in spite of* the factors of adaptation and fatigue. The effects of adaptation and fatigue were probably always real, and somewhat constant. The emotional factor was not *necessarily* present at all times; and all indications point to the conclusion that it operated irregularly, and *most of the time not at all.* It is hard to believe that the emotional factor played a very important part, when such definite contraction effects were secured in every case. It was obviously much *weaker* than the conditioned reaction which was established. Since the dilating effect had to be obtained in spite of these rather constant factors of adaptation and fatigue, and since it was obtained regularly in *every* case, I am inclined to believe that a real connection was formed between *dilatation* of the pupil and the conditioning stimulus. Further evidence in favor of this same conclusion is found in the next experiment, in which a dilatation of the pupil was conditioned to an electric shock, with a reliability of practically 100 per cent.

Table VII. gives the results of an experiment with subject *F*, in which a contraction, and later in the same experiment a dilatation, of the pupil was conditioned to an electric shock. The electrodes were two cups of salt water, and the subject kept the first two fingers of his right hand in one cup and the last two fingers of his right hand in the other cup. The procedure in the first part of this experiment was to turn on the light and give the electric shock while the

TABLE VII.

SUBJECT *F*, MCH. 5, 1:30 P.M.–5:00 P.M.

Before Training	(*a*) Light	Meas.	13	
		Av.	39.5	±0.42
		S.D.	2.24	
	(*b*) Light and shock	Meas.	13	
		Av.	40.8	±0.39
		S.D.	2.08	
	(*c*) Light and bell	Meas.	13	
		Av.	40.8	±0.30
		S.D.	1.61	
	Shock, or $b - a$		+1.3	±0.57
	Bell, or $c - a$		+1.3	±0.52

Training: Shock with contraction. 200 repetitions. Bell not used.

After Training Towards Contraction	(*d*) Light	Meas.	14	
		Av.	39.9	±0.41
		S.D.	2.29	
	(*e*) Light and shock	Meas.	14	
		Av.	36.4	±0.33
		S.D.	1.84	
	(*f*) Light and bell	Meas.	4	
		Av.	40.5	±0.02
		S.D.	0.05	
	Shock, or $e - d$		−3.5	±0.53
	Bell, or $f - d$		+0.6	±0.41

Effect of training (shock), or $(e-d)-(b-a)$ = ... −4.8 ±0.78
Reliability = ... 100/100

Difference in effect of bell, or $(f-d)-(c-a)$ = ... −0.7 ±0.66
Reliability = ... 76.06/100

Training: Shock with dilatation. 200 repetitions. Bell not used.

After Training Towards Dilatation	(*g*) Light	Meas.	11	
		Av.	39.4	±0.46
		S.D.	1.15	
	(*h*) Light and shock	Meas.	13	
		Av.	45.5	±0.33
		S.D.	1.79	
	Shock, or $h - g$		+6.1	±0.57

Effect of training (shock), or $(h-g)-(b-a)$ = ... +4.8 ±0.81
Reliability = ... 99.997/100

Effect of difference in training, or $(h-g)-(e-d)$ = ... +9.6 ±0.78
Reliability = ... 100/100

In the above table, the conditioning stimulus was an electric shock applied to the subject's right hand. In the first training period, the shock stimulus was present while the pupil was contracting. After the first conditioned response had been 'worn out,' the bell was used to test the effect of 'just any stimulus' on the pupil. In the second training period, the shock stimulus was present while the pupil was dilating. A contraction, and later a dilatation, of the pupil was conditioned to the shock, in the same experiment. The measurements of distributions *a*, *b*, and *c* were mixed up together; likewise those of *d*, *e*, and *f*; and also *g* and *h*.

pupil was contracting. After some 200 repetitions of the training stimuli had been given, I found that the conditioned reaction was already fairly well established. So I decided to take some measurements, and these are represented by distributions *d* and *e*. The effect of training towards contraction, with the shock, is given by the figure -4.8 ± 0.78. The reliability of this difference is approximately 100 cases out of 100.

I next called out the conditioned reaction several times, and when it seemed to have temporarily disappeared, I rang the *bell* to see what effect might be produced. I rang the bell 4 different times (distribution *f*), and found that the bell now caused less dilatation of the pupil than at the beginning of the experiment. The difference is represented by -0.7 ± 0.66; which is not very reliable. This difference in effect of the bell might be explained by a difference in a state of the subject caused by the experience of going through the first part of the experiment. The bell did not contract the pupil. It simply dilated it less than it had done before. No particular conclusion can be drawn from this phase of the experiment.

During the latter part of this experiment the training was in the other direction; the light was turned off and the shock stimulus was given while the pupil was dilating. After some 200 repetitions of this compound stimulus, a *dilatation* of the pupil was definitely conditioned to the shock. The results of this whole experiment, obtained with this reversal of procedure in the same subject and at the same sitting, are very definite and convincing. The difference between training towards contraction and training towards dilatation comes to 9.6 ± 0.78; and it is positively certain that a difference in training will produce a different effect. It appears from this experiment that the pupillary reflex can be conditioned to a shock somewhat more readily than to a sound. But I have not worked with shocks enough to be very certain that this is true.

Practically nothing has been said so far about the matter

of rhythm. If the pupil is caused to dilate rhythmically, for example, every 15 sec. during the training period, and, furthermore, if the light change (from bright to dark) is now omitted, it might be expected that the pupil would dilate somewhat after 15 sec. had elapsed. Although there is no positive evidence that such a result can be secured in the case of the pupillary reflex, this is a factor which should always be guarded against. Although there was a very decided rhythmic effect (as regards the *time* of giving the stimuli) in the training series, the presence of this factor would not in any way vitiate the conclusions reached. For example, in the procedure of Table I., the rhythmic effect was for the pupil to contract and then to dilate. This rhythmic effect had exactly the same opportunity of operating when the bell was ringing as when it was not ringing. The same thing is true for all other procedures used: this rhythmic effect had very little opportunity to function, and had exactly the *same* chance of operating when the bell stimulus was present as when it was not present.

From the results of the above experiments, it may not be entirely clear whether the bell was connected with a change-in-size-of-the-pupil or with a state of the pupil. In all experiments where the training was towards contraction, the light stimulus was present while the bell was ringing. It is quite possible that the constant light stimulus and the auditory stimulus became 'integrated.' We might say that the two had come by habit to form a 'situation.' *With* the auditory stimulus, the light stimulus is now more at home; *without* the auditory stimulus, the light stimulus does not work so 'smoothly.' Furthermore, the same thing is true of all experiments where the training was towards dilatation. The positive darkness stimulus was always present when the bell stimulus was present. In these cases, the bell stimulus and the darkness may have become 'integrated.' *With* the bell, the darkness now functions with fewer 'disturbances'; *without* the bell, the darkness is 'ill at ease.' This 'integration' may even have been made at a higher level in the nervous system. This interpretation, even if correct, would

not influence any of the conclusions which have been reached. In Table IV. the bell was present while the pupil was in a *state* of contraction. This was the experiment in which the bell came immediately after a contraction and immediately before a dilatation of the pupil. The results of this particular experiment furnish evidence *against* the idea of the bell being associated with a *state* of the pupil; but, also, it does *not* furnish any evidence for the assumption that the bell stimulus became associated with a change in the size of the pupil. Evidence for this last assumption, however, is furnished by every other experiment which has been performed.

In the above discussion, I have included the results of *all* experiments which were conducted with the procedure as outlined in the previous section. I have not discussed the somewhat irregular results of some of the 'preliminary' experiments, because my early procedure was not satisfactory. In the first place, the training period was much too short; and, in the second place, I attempted to condition this reflex by giving the training on successive days. The irregular and contradictory results obtained were due in part to insufficient training, and in part to the fact that any association which might have been formed on one day was probably unlearned before the training was renewed on the following day. All indications are to the effect that memory for the conditioned pupillary reaction persists for a very short time. This is no doubt partly due to an active unlearning process. While the results of the preliminary experiments do not deserve a detailed discussion, these experiments do furnish some information as to the best procedure to follow in conditioning the pupillary reflex. The requirements which this procedure should meet, as I see it, are as follows:

1. The training should consist of a great many repetitions of the training stimuli, given continuously.

2. Due attention should be paid to the matters of fatigue and adaptation. The subject should be given frequent rests, to allow the pupil to function properly. Occasionally a rest of a few minutes should be given to allow the subject to relax. There will be less general fatigue of the subject if some schedule is followed rigidly.

3. The conditioning stimulus must not be given except when the fundamental stimulus is present. In other words, no 'exceptions' to the training must be allowed. It is probably advisable also not to allow the pupil to change markedly (in the direction of the training) except when the conditioning stimulus is also present, though there is no conclusive evidence favoring this assumption.

4. Disturbing light and sound stimuli especially must be avoided.

Section 6

Conclusions

The main conclusions which I feel justified in drawing from this experiment are as follows:

1. The pupillary reflex can be conditioned to the sound of an electric bell, to the buzzing sound of a telephone receiver, or to an electric shock.

2. A contraction of the pupil is more definitely conditioned than a dilatation. The evidence is conclusive that a contraction of the pupil was conditioned; and it is practically certain that a dilatation of the pupil was also conditioned.

3. The conditioning stimulus may eventually cause a change in the size of the pupil which can be readily distinguished by the observer. With the procedure used, this change generally amounted to about 0.5 mm. (in horizontal diameter); but it differed widely in different individuals and in the same individual at different times. But even after a long training series, the conditioned reaction disappeared very quickly.

4. A conditioned reaction is readily established when the bell stimulus is given *during* a change in the size of the pupil. Every evidence is found in favor of this conclusion. No very good evidence is found in favor of the supposition that a conditioned reaction may be established when the bell stimulus is given *before* the change in the size of the pupil. This possibility, however, is not eliminated. Positive evidence is found against the forming of a connection when the bell stimulus is given *after* the change in the size of the pupil.

This is to say, no evidence is found for the forming of associations in the backward direction.

5. Positive evidence is found that a change in the size of the pupil can be conditioned to a specific stimulus, and not to just any stimulus affecting the nervous system at the time. In two experiments, the sound of a bell and the buzzing sound of a telephone receiver were distinguished from each other. In one of these experiments, a contraction of the pupil was conditioned to a telephone receiver, and a dilatation of the pupil to a bell; and, in the other experiment, a contraction of the pupil was conditioned to a bell, and a dilatation of the pupil to a telephone receiver. The nervous system did not confuse these two stimuli.

6. Negative auditory adaptation, fatigue, and an emotional reaction are the principal disturbing factors in this experiment. These factors, however, do not vitiate any conclusions which have been reached, because both contracting and dilating effects were produced. These three factors operated in the same fashion when the training was towards dilatation as when the training was towards contraction. If their effects were considerable when the training was towards contraction, then their effects were small when the training was towards dilatation; and vice versa. The procedure used gives a reliable measure of the limitations of these three factors in explaining the results.

In the above discussion and summary of results, no claim has been made that the particular mechanism of the conditioned reaction secured was at all clear-cut or well-defined. While the original stimulus and the end reaction are definitely known, what intervened between the two is partly a matter of speculation. It is not known just what 'secondary' reactions are called out in normal people by an auditory stimulus. It is therefore quite possible that in the present experiment some such reaction or reactions intervened between the innervation of the cochlear nerve and the innervation of the pupillary fibers. The fibers of the pupillary system may

have been affected at several different points along their course. There is also good reason to believe that in the training towards contraction and also in the training towards dilatation, the tonus of *both* the sphincter and dilator systems were affected. It may not be wise, therefore, to attempt to localize the particular part of the anatomy where the 'conditioning' took place, because the process was probably diffused and complicated. Still it will not be entirely out of order to make a guess at the structures which probably played a major part in learning this conditioned reaction. Since the bell did not contract the pupil at the beginning of the experiment, and since the bell did contract the pupil at the end of the experiment, it is evident that some terminal axones from the cochlear system have been brought into functional activity with certain cell bodies whose processes go to the muscles of the iris. This is certainly *not* speculation. To explain this result, we simply need some terminal endings from the cochlear nerve situated near some cell bodies of the efferent pupillary fibers. The oculomotor nuclei are suggested at once. This seems to be a likely place where *some* of the conditioning may have taken place, since some of these nuclei have to do with the reflex turning of the eyes to sound, while other oculomotor nuclei are ordinarily involved in contracting the pupil. There may be cross connections in these nuclei, but this is not definitely known. Furthermore, it is likely that a large part of the 'conditioning' was done by the cerebrum, especially the temporal lobe. But these two suggestions may prove misleading, since various secondary reactions may have been produced.

The results of this experiment are very suggestive in view of the fact that the pupillary reflex is not subject to voluntary control, and also in view of the fact that this reflex occurs in the human fœtus of 6 months (12, 241), and in sharks several hours after death (10, 39–40). The experiment, taken as a whole, emphasizes the reflex nature of the human organism. It is generally known that many reflexes are 'nonconscious,' and that various acts may easily become nonconscious with repetition. For example, one quickly

learns to write 'the' on the typewriter without thinking of the three separate elements, t, h, and e. It is soon reacted to as a whole. In *learning*, however, it is often assumed in one way or another that conscious processes are *necessarily* involved. In the above experiments, the subjects may have been conscious sometimes of the conditioning stimulus, but they were not conscious of the changes which were taking place in their pupils. Ordinarily, we are conscious of the movement we are learning; but in this experiment the subjects were not conscious of the *act* which was learned.

It would not be correct to assume that a natural 'tendency' for the bell to *contract* the pupil was present already. The only 'tendency' present was for the bell to dilate the pupil. The nerve fibers involved in learning this conditioned reaction were already present, to be sure, but they were not already in functional activity. The experiment is suggestive of the fundamental mechanism of all associations. Just 'fulfill the requirements' of the training period of a given conditioned reaction, and there is no a priori reason for believing that a connection will not be formed. This experiment is suggestive of the way all learning may take place.

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THE EFFECTS OF REPETITIONS UPON RETENTION

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At the suggestion of Professor Pillsbury and under his guidance, I repeated a part of the experiments on memory done by Ebbinghaus. Consequently the brief summary of the part of his work is the necessary introduction to my work.

Ebbinghaus,[1] while he was doing very extensive experiments on memory, noticed the fact that as the number of readings increases, the series are engraved more and more deeply and indelibly; if the number of readings is small, the inscription is but surface deep and only fleeting glimpses of the tracery can be caught; with a somewhat greater number of readings, the inscription can, for a time at least, be read at will; as the number of readings is still further increased the deeply cut picture of the series fades out only after ever longer intervals. Then he thought that the inner stability of a series of ideas, or degree of its retainability means the greater or less readiness with which it is reproduced at some definite time subsequent to its first memorization. This readiness is measured by the amount of work saved in the relearning of any series as compared with the work necessary for memorizing similar but entirely new series.

From this conclusion he devised the Savings Method. For material, he used series of nonsense syllables, each series consisting of sixteen syllables. These series are impressed to definite extents as a result of different numbers of readings, and then twenty-four hours later they are learned to the point of the first possible reproduction by heart, and he found how resulting savings in work are related to each other and to the corresponding number of the former readings.

[1] Ebbinghaus—memory 1913.

The first reading was repeated 8, 16, 24, 32, 42, 53, 64 times, and for each, ten tests were made. He worked as subject and experimenter himself. The learning curve in this method is almost a straight line within this limit,—zero on the one hand and on the other 64, which is almost double the number of readings that is sufficient for him to learn the series and beyond which he found himself unable to go, owing to headache and fatigue caused by too long concentration.

Ebbinghaus used such big intervals in numbers of the first readings as 8, 16, 24, and so on, and did not see what would happen between 8 and 16, 16 and 24, and so on, granting that the lines between these points are straight. My experiment is to find whether the line between 0 and 10 repetitions is straight or not.

My material consisted of series of nonsense syllables, each series consisting of twelve nonsense syllables and each syllable consisting of two consonants and one vowel between the consonants, avoiding syllables difficult to pronounce.

Subjects[1] are all trained subjects, four graduate students and two seniors in psychology, four of whom were women and the rest men.

Each series is put on the drum and is shown to the subject one syllable at a time through the window of the screen before the drum, the rate being approximately two seconds per syllable. The subject is told to concentrate on the syllable shown in the window at the time and to try to memorize it but not to make any special associations between syllables or with any thing else. The learner was free to read aloud or silently or to move head or hands as syllables were read. In this way the series is shown twice successively and then there was one hour intermission, during which he is free to do anything but recall the syllables just read. At the end of one hour he comes back to me and recalls as many syllables as he can and I take the records of them. Then the

[1] I wish to express my great appreciation to the subjects who worked so earnestly and patiently in memorizing nonsense syllables for me. If the result of the work is a contribution to psychology, all the compliments should be given to my diligent subjects, Miss Flynn, Miss Gordon, Miss Guernsey, Miss Palmer, Mr. Page, and Mr. Skaggs.

same series is shown in the same way as before but this time it is shown as many times as he needs in order to give the first possible reproduction of the series without a mistake in spelling and in order.

In order not to give more readings than adequate for the first possible reproduction, I let the subject recite the series after every three readings, provided the subject should tell me if he thinks he memorized the series before the three readings were over. I adopted this method because I did not see how a subject could tell whether or not he had just adequate readings for a perfect reproduction if no chance of trial is given, and even when he thinks he memorized all syllables, he may not be able to recite perfectly when he tries, then I must give him more readings, and that trial of reciting will engrave the already acquired syllables and thus influences the number of readings. Some subjects may be too conscientious and do not dare to recite until they feel sure of them, thus they may have more readings than necessary. To avoid this irregularity I took the methods of three readings and one recitation. Later I found that I did not need to fear this irregularity so much because when subjects were practiced in the work they could tell when they memorized the series pretty definitely. But it was too late to change the method when I learned it so I continued to use it all through the experiments for every subject. I took records of numbers of readings and recitations and syllables at each recitation. The time for recitation was not limited.

The same test was repeated with different series each time and with different numbers of first readings. The number of first readings varied as 2, 4, 6, 8, 10. The time of experiment for the same subject was almost constant each day. The place of experiment was my private laboratory where external disturbance was at a minimum.

The results for each individual are gathered in the following table. At the top of each double column is the designation of the worker. Under Rep. for learning is given the number of readings originally made. Under Readings and Recitations stands the number of readings plus attempted recitations

required to learn the series after one hour. In the column headed 'saved' is given the difference in number required between the material read twice and the material read four times.

TABLE I

Rep. for Learning	F.		S.E.		S.O.		Pg.		Pl.		S.	
	Readings and Recitations	Saved	Readings and Recitations	Saved	Readings and Recitations	Saved	Readings and Recitations	Saved	Readings and Recitations	Saved	Readings and Recitations	Saved
2.....	6.9		8.2		11.3		10.2		8.4		14.8	
4.....	7.3	−0.4	7.9	0.3	9.2	2.1	8.6	1.6	6.2	2.2	9.6	6.2
6.....	5.5	1.8	5.4	2.5	7.2	2.0	8.0	0.6	6.9	−0.7	10.7	−0.9
8.....	5.5	0	4.8	0.6	5.2	2.0	7.9	−0.1	5.2	1.7	11.3	−0.6
10.....	5.9	−0.4	2.8	2.0	5.2	0	7.4	0.5	4.2	1.0	9.7	1.3
No. of series ...	10		10		6		10		10		7	

As will be seen, the records are for the most part highly irregular. In S. and F. we have individuals who find it difficult to give attention to nonsense syllables, and who object to the work. They are most irregular, but no one shows any approximation to a straight line or to the type of curve reported by Williams. Any attempt to treat them mathematically would not be worth the time expended. Instead we attempted to smooth out the curve by averaging the results from all subjects, bad as that may be in theory. The results give a regularity nearly equal to those obtained by Ebbinghaus, although the combination from so many diverse individual reports is obviously open to objections.

Bringing all the means of numbers of readings in re-learning of all subjects together the following table is obtained.

The increases of the numbers of the first readings (Column I.) and of the work saved (Column II.) in each step are in the same ratio, and the number of readings saved for each first reading (Column IV.) is constant except at one point and consequently the curve is almost a straight line.

Sometimes some subjects had more recitations than the proportion of one to three readings and so adding those

TABLE II

I	II		III		IV
No. of First Readings	No. of Readings in Relearning	P.E.	No. of Readings Saved	P.E.	No. of Readings Saved for Every First Reading
2.....	6.89	.15			
4.....	5.70	.11	1.19	.13	.30
6.....	4.91	.12	1.98	.06	.33
8.....	4.51	.17	2.38	.07	.30
10.....	3.89	.16	3.00	.13	.30

$M = .31$

recitations to readings will be fairer. The following table will show the result of readings and recitations required together.

TABLE III

I	II		III		IV
No. of First Readings	No. of Readings and Recitations in Relearning	P.E.	No. of Readings and Recitations Saved	P.E.	No. of Readings and Recitations Saved for Every First Reading
2.....	9.60	.18			
4.....	8.00	.14	1.60	.16	.40
6.....	7.09	.17	2.51	.07	.42
8.....	6.49	.22	3.11	.10	.39
10.....	5.70	.23	3.90	.18	.39

$M = .40$

In this table the result is almost the same as in the first, with slightly smaller deviations.

Our results indicate that the straight line relation which Ebbinghaus found to hold for the moderate and large number of repetitions holds also for the earlier repetitions, the values below his initial value. Ebbinghaus found that each three repetitions saved approximately one repetition after twenty-four hours. Our values are approximately the same.

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